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THE PSYCHOLOGICAL REVIEW

A NEEDED EMPHASIS IN PSYCHOLOGICAL RESEARCH [1]

BY C. C. ROSS

University of Kentucky

Professor H. L. Hollingworth in his presidential address to the American Psychological Association at Columbus in 1928 warned against ignorant pseudo-psychologists who knew how to pronounce the word *psychology* but not how to spell it. Apparently the time has come when it is necessary to warn against the practice of psychological research by persons who may know how to spell the term but who appear ignorant of its derivation and meaning.

According to the dictionary the word *research* comes from the Latin prefix *re-*, meaning *again*, and the French word *cerchier*, meaning *search*. The dictionary places the accent on the second syllable, thus authorizing the pronunciation *re search'* rather than *re'search;* and much of our so-called psychological research reveals clearly that psychological practice is in line with the dictionary pronunciation rather than the derivation. Psychology has unmistakably emphasized the *search* and overlooked the *re*. Indeed much psychological experimentation might more properly be described as psychological *search* rather than psychological *research*.

At a meeting of the American Statistical Association a few years ago Frank M. Phillips, of the Bureau of Education at Washington, declared that educational research had really never settled anything, for the investigator merely carried the

[1] A paper read before the Southern Society for Philosophy and Psychology at Nashville, Tennessee, April 20, 1935.

study far enough to enable him to formulate a tentative conclusion and to assert that the matter warranted further study—and then he promptly dropped it! Psychological research has differed from educational research not so much in the speed of its reaction time in letting go of a topic, no matter how promising, after a preliminary and often sketchy study, as in the fact that its conclusions have not always been recognized as merely tentative.

Perhaps it should cause no surprise in a new nation such as America, whose debt of gratitude to such dauntless pioneers as Daniel Boone and David Crockett must be forever great, that there should have developed for pioneer fathers in any field what a Freudian might call a sort of father fixation, or possibly a 'covered-wagon complex.' Be that as it may, psychological research has overemphasized exploration and has underemphasized verification. Not only must candidates for the doctorate in psychology, for example, contribute some bit of original research but the repetition of an earlier experiment has usually been considered beneath the dignity of a self-respecting master of arts candidate.

A single example of this undue prestige of the pioneer, this 'covered-wagon complex,' must suffice. Louise Seibert (**15**), of Goucher, in a monograph published in 1932 says:

> We could not help being impressed while going over the work done in our particular field [studies in learning and forgetting] by the need of a thorough overhauling of the mass of data that have been accumulating since the pioneer days of psychological investigation. We still go back to the beginnings of experimentation, and we accept all the findings: Experiments made on one individual, or a group of three or five; experiments with no control whatever; experiments where the technique is so faulty or the interpretation so obviously wrong that the value of the findings are almost nil. These early experiments are accepted as well as the most recent significant ones, and are given a place in our books on psychology and their findings are influencing our methods of teaching. . . . In our constant research for original investigations, we do not relish repeating experiments done by others.

In the above quotation Seibert had especially in mind the experimental work of Ebbinghaus on nonsense syllables.

Doubtless no better example of the magic potency of the pioneer could be found. In the forty years which have intervened since Ebbinghaus published his epoch-making work, his experiments have been repeated on a more elaborate scale by Müller and Schumann, and more recently by Cuff and others; and all have shown significant differences in result. However, so great is the prestige of the pioneer that these later experiments are commonly overlooked and only that of Ebbinghaus is reported in textbooks on educational psychology. But note the absurdity of this procedure. Here we have the results of one man (and that man a genius if there ever was one) who worked upon meaningless material, and who was at the same time experimenter and subject. Now note what happens. The applied psychologist, like the magician, takes this single experiment, puts it in a hat, so to speak, and by a slight-of-hand trick that outdoes Houdini extracts all sorts of ducks and rabbits. The excited and admiring audience is shown all that it needs to know about learning and forgetting—whether for names or faces, for shopping lists or golf scores, for telephone numbers or French vocabularies, as well as for the wife's birthday and where you put your summer underwear. Surely, here is the world's record in the inductive leap!

In a review of Murphy's 'Experimental Social Psychology,' Mark May (**8**) says:

> Perhaps the most outstanding impression which this book leaves with the reader is the spotty nature of the experimental data in social psychology. . . . Another striking fact is that so few experiments are repeated by other investigators. That which is so common in the physical sciences rarely happens in the social sciences. The result is an increasing accumulation of unverified data.

Here, then, is the sad result of this glorification of the pioneer spirit in psychology, this persistent seeking after something new.

The writer now wishes to make a plea for a much needed emphasis in psychological research, namely, upon verification instead of upon mere extension. The picturesque pioneer has

his rightful place when the land is new, and unbroken prairie and virgin forest appear to stretch out endlessly ahead. But there comes a time for the permanent settler and the patient tiller of the soil. In the writer's opinion we have reached this place in psychological research today. Here, as elsewhere, rugged individualism must give way to some sort of coöperative enterprise.

The outstanding weakness of modern psychology is a scarcity of fact and an oversupply of theory. Heidbreder (**6**), for example, in a recent summary of seven so-called 'schools of psychology' makes the disturbing statement that "there is not enough fact in the whole science of psychology to make a single solid system." Psychology needs first of all to know what alleged facts really are facts. Only when this is done will we be in the position to judge of the validity of theories which purport to explain these facts. This will require the careful repetition of experiments which are regarded as significant, either because of their influence upon psychological theory or because of their practical applications. A brief consideration of the present status of the transfer of training will illustrate the limitation of much existing psychological research and of the theory so closely interwoven with it.

In 1928 Whipple (**16**) analyzed the experimental literature on this topic and arrived at this major generalization:

> In the light of what we now know to be necessary for reliable inference, many of the experimental studies of transfer are so inadequate in scope and method that they afford altogether shaky ground for establishing permanent conclusions.
>
> Thus the subjects have been too few in number or already too well 'crystallized' mentally to expect any marked transfer of improvement. . . . Again, there has been often no control group or there has been no effort to compute statistically the extent to which mere chance has affected the result. . . . Finally, many of the investigators have picked for their subjects a half dozen graduate students or instructors, who in the very nature of the case, represent a type of mind that has been rigorously selected for having already a superior stage of mental training.

That the first three limitations mentioned, namely, too few cases, inadequate control, and faulty statistical analysis,

are serious in much of psychological experimentation no careful student will deny; and yet the writer is inclined to think the fourth limitation, namely, a highly selected group of subjects, is even more dangerous because it is so often overlooked.

At first glance it seems surprising that psychology, which is in the main responsible for the many studies of individual differences from Galton's day to the present, should have been so slow to appreciate the implications for experimental method of the almost infinite variation of human personality. And, yet, perhaps this was inevitable. "Every new science," says Bertrand Russell (**14**), "is hampered by too slavish an imitation of the techniques of some other science." That is just what has happened to psychology. In the middle of the last century those who gave set and form to the new science were men trained in other sciences, and, as was natural, they carried over into psychology the presuppositions and techniques found fruitful elsewhere. "All the pioneers in experimental psychology," says Barlett (**2**), "were trained either in physics or physiology. Their influence, both for good and for ill, still remains stamped upon the accepted methods of the psychological laboratory." It is not too much to add that neither in the experimental procedures for securing its data nor in the techniques of analysis has proper recognition been given to the almost infinite variability of human personalities from each other at any given time, much less of the variation of the same personality from day to day and even from situation to situation.

With regard to the highly selected character of the subjects used in many laboratory experiments, Flügel (**4**) says:

> Only those who are willing to sacrifice ease and dignity . . . or those who are woefully deficient in a sense of humor will be ready to submit their minds and bodies to the experimenter. . . . No wonder that experimental psychologists are thrown back upon themselves.

In fairness to the author it should be noted that this is merely an explanation and not justification. Furthermore, if the experimenter would be content with drawing generali-

zations with reference to adult but embryonic psychologists, little harm would be done. But this is rarely the case. To generalize from students with a special interest in psychology to other adults, much less to children, is to argue by analogy. On this point Truman Lee Kelley (7) says:

> It seems no overstatement to say that argument by analogy has been the greatest cause of error and the greatest restriction of vision of scientists of all times. . . . We cannot do away with it, but it should be employed only in the tentative solution phase of a problem.

Doubtless the most hazardous form of analogy is from animal psychology to human application, as is so often done in educational psychology. Yet this is based upon the very dubious assumption that what may be true of a rat in a maze or a cat in a puzzle-box will be equally true of a kindergarten child in the sand box or a high school boy tackling the mysteries of algebra. Lloyd Morgan's cannon appears indeed to be a coast defense gun which fires in one direction only. It has seemed rather effective in keeping psychologists from making men out of monkeys, but it has not been so successful in preventing psychologists from making monkeys of men. No matter if Lashley's elaborate experiments on rats and monkeys, for example, should have established beyond doubt the equipotentiality of the cortex in these lower animals; they do not thereby automatically establish the same for man. As a native of Tennessee I experience no more difficulty in believing the hypothesis that in the evolution of man he has exchanged a prehensile tail for a more flexible hand than that he has at the same time changed his generalized brain for one more specialized in character.

Perhaps some psychologists may share the skepticism of the value of repetition in psychological research, which Peterson (10) expressed in his recent presidential address to the American Psychological Association regarding the value of repetition as a 'general panacea for learning as well as for retention.' Very well, then, let us have the 'completeness of response principle' instead. Peterson himself fully realizes the necessity of repetition of experiments, even of the crucial

one which is apparently to show that repetition alone is itself of little value. And, apparently when psychology has learned by repetition that repetition is of no value in learning, we shall be in position to appreciate fully the declaration of the professor of logic who declared: "All general statements are false, including this one!"

What is needed in psychological research is not mere blind repetition, but repetition with insight. Let us take a cue from Woodrow's (**17**) experiment on transfer. It will be recalled that in addition to the control group Woodrow had two experimental groups, one of which spent the time in unenlightened practice or repetition whereas the other combined practice with instruction for the purpose of gaining insight into the process. The results showed a striking superiority for the group which combined repetition with insight. To be effective the repetition of strategic psychological experiments must be illumined by insight.

As an illustration of the values of such repetition let us consider briefly the work of Adams (**1**) in repeating Thorndike's famous experiments with the cats and puzzle-box. Although he followed the original technique as faithfully as the descriptions would permit, certain important differences in results were apparent. For example, instead of the excited, indiscriminate clawing, scratching and biting observed by the then youthful Thorndike, Adams' cats took a far more leisurely attitude toward life, thus suggesting the inadequacy of time as a measure of learning. Moreover, only one animal showed what might be termed a gradual elimination of useless movements. But especially important perhaps was the versatility of the animals, once the trick had been discovered. One cat, for example, pulled the loop to which the string was attached four times with the paw, twice by being held between the chin and the paw, once with the teeth, once with the teeth and the paw together, and the like. This hardly looks like the mechanical 'stamping in' of the successful response when accidentally hit upon.

Let there be, then, more repetition of significant experiments in psychology. In this manner alone will we find the

true facts as well as the respective merits of the rival hypotheses which seek to explain them. We shall thereby be 'killing two birds with one stone.' And incidentally, I am inclined to think we shall also be crippling a third bird, or at least restricting somewhat its range of flight.

Let me explain what I mean by this. When a serious and widespread effort is made to verify significant experiments, one outcome will doubtless be the discovery that the laboratory experiment itself has certain limitations. The first of these grows out of the attempt at rigid control of the laboratory situation. Murphy (**9**) says: "Much of the social behavior which is the actual marrow of the social sciences would not or could not occur in the artificial situation, in which the conditions were determined by the experimenter." And, as we are increasingly realizing that the experimenter is always a part of the experimental situation, practically all laboratory responses must be classified as social behavior (**11**). Florence Goodenough (**5**) in her recent book 'Developmental Psychology' says:

> Minor experiments, to be sure, can be carried out in the laboratory under fairly well controlled conditions. . . . But if we . . . hope to make psychology contribute to human welfare and individual happiness to an extent that is comparable with other sciences, we must cease to content ourselves with little studies of isolated bits of behavior and turn to the major problems of differential development under varying conditions and over long periods of time.

A recent personal experience of the writer convinces him that to generalize from a laboratory experiment to a classroom application involves a dangerous analogy. Many laboratory experiments, including some by the writer (**12**), have shown a rather definite and consistent relationship between a knowledge of progress and achievement in learning. In fact, so convincing has the evidence appeared that practically all recent textbooks in educational psychology have assumed that the same relationship exists in the classroom, and that a knowledge of progress is one of the surest motivation techniques available to the teacher. However, when the writer attempted to test this hypothesis with ordinary informal

achievement tests in a college class three years ago, the results were consistently and convincingly negative (**13**). Nowhere was there a statistically significant difference. Not content with these results, the writer has since continued the study with two other classes in the same subject and with one in a different subject. He has also persuaded a colleague in another department to do the same. In all, these groups involved more than 50 tests and about 300 students, and not once has there appeared a difference favoring the group with full knowledge of progress that meets the minimum requirement for statistical significance, namely, that it be three times its probable error.

The conclusion appears to the writer inescapable. The Gestalt of the laboratory situation is so different from that of the life situation outside that it is hazardous to generalize from one to the other. One can never be certain what the outcome of a laboratory experiment will be when applied to the classroom situation until it has been actually tested out in that situation. The practical implications of this conclusion are sufficiently evident when we consider it alongside a statement made by Carr (**3**) less than two years ago: "We know much about learning, retention, and recall for our usual laboratory conditions, but relatively little of these phenomena as they occur in actual life."

My plea is, then, for a return to the original meaning of the word *research* (with an emphasis on the *re-*). Psychology has had too much *search* and not enough *re-*. Novelty may be noteworthy, but let us make repetition also respectable. Let us search again and again, progressively modifying the laboratory conditions until they approach those of real life.

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[MS. received July 22, 1935]

THEORIES OF RETROACTIVE INHIBITION*

BY STEUART H. BRITT

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Three major theories have been offered to account for retroactive inhibition: the perseveration theory of Müller and Pilzecker (**23**), the transfer theory of DeCamp (**7**) (essentially a modified perseveration theory), a combination of the transfer hypothesis and a disruption hypothesis by Webb (**31**). These theories have been considered by various experimenters in interpreting the results of their studies in the field of retroactive inhibition. The factual data are regarded by one investigator as favoring somewhat a perseveration theory, at least during the first few hours after learning (Skaggs, **28**, **29**, **30**); by another as favoring a transfer theory (Robinson, **25**); and by a third as not being crucial thus far and hence indecisive as regards a transfer or a perseveration hypothesis (McGeoch, **15**, **16**, **17**; McGeoch and McDonald, **18**; McGeoch and McKinney, **19**, **20**; McGeoch and Nolen, **21**). The three major theories of retroaction together with the views of these other investigators have previously been summarized by Britt (**2**). It is pertinent, therefore, that the theories be examined as to their adequacy, with a view to setting forth a satisfactory theory of retroactive inhibition.

The perseveration theory by itself is not sufficient to account for retroactive inhibition. The writer would not imply that the assumption made by Skaggs (**29**), "that there is a perseveration of the biochemical processes in neurones and synapses even after the original stimulus is removed," is necessarily incorrect. The writer does maintain, however, that an interpretation of retroaction as due *simply* to an interference with this process and drainage of the "energy

* This article was read in part on December 30, 1935, before Section I of the American Association for the Advancement of Science in St. Louis.

from the neurones which were originally excited" (29) is not a *sufficient* explanation.[1] At least three sets of experimental data can be marshalled against the adequacy of such a view.

In the first place, certain of Müller and Pilzecker's (23) results suggested that interference with the processes perseverating from the original activity was a function of the strength or intensity of the subsequent activity. There was no indication of the possibility of a relationship between amount of retroactive inhibition and the degree of similarity of the interpolated activity to the original activity. It was apparently to meet this limitation that DeCamp (7) suggested his modification, 'the relative identity of the neurological groups concerned.' Experimental work subsequent to DeCamp's study has shown that the similarity of the two activities is a highly important factor, *e.g.*, the results of Webb (31), Robinson (25, 26), Skaggs (27), Johnson (13), Gengerelli (9), Gibson and Gibson (10), among others.[2]

In the second place, since the essential point of a perseveration theory involves a 'setting-in' of neural processes for only a short time upon completion of learning, the subsequent activity would have to follow the original activity immediately or after a very short interval (at most, a few minutes or hours) in order to exert a retroactive influence. This is untrue, in that retroactive inhibition has been demonstrated when the interpolated activity has not been introduced until several hours, days, or even weeks after original learning, *e.g.*, Whitely (32), Bunch and McTeer (4), Lester (14), Bunch and Wientge (5), Britt and Bunch (3), and McGeoch (17).

In the third place, the assumption seems of doubtful validity that the interference effect from the interpolated activity varies inversely as the length of the time interval between the original and the interpolated activity—in other words, that the earlier the interpolated activity is engaged

[1] Perhaps Skaggs would agree to this; *cf.* his recent views (30).

[2] The results of McGeoch and McKinney as to the potency of nonsense syllables to inhibit poetry to the same extent as more poetry of the same kind (19), or to inhibit prose almost to the same extent as more prose of the same kind (20), have been interpreted in terms of similarity *and* 'degrees of organization.'

in after memorizing, the greater is the amount of retroactive inhibition. This assumption is questionable in the light of the results of experiments concerned with the temporal position of the interpolated activity, by Robinson (**25**), Whitely (**32**), McGeoch (**17**), and McGeoch and Nolen (**21**).[3]

DeCamp's (**7**) theory, that "retroactive inhibition would vary directly as the relative identity of the neurological groups concerned," insofar as it is based upon a theory of 'after-discharge' of neural elements, is open to objection as a modified perseveration theory. As to his stress of the matter of 'relative identity,' the similarity factor is most certainly important, but DeCamp's statements are too general to be entirely satisfactory.

The hypotheses offered by Webb seem much better from every standpoint. The writer feels that a development of his theory (together with a realization of the importance of the fact that underlying neural processes are involved) may prove to be a satisfactory explanation of retroactive inhibition. The remainder of the present article represents an attempt at an elaboration of Webb's hypotheses.

Webb discussed both 'transfer' and 'disruption.' Although he admitted that both might operate, the general tenor of his evaluation of his own results was that transfer, and not disruption, was responsible for retroaction. In a certain sense his theory may be regarded as essentially a transfer hypothesis throughout; that is, it involves a transfer of some phases of the original activity to the interpolated activity, and also a transfer of some phases of the interpolated activity to the relearning of the original activity. The first transfer results in the partial disorganization of the original activity, at the time of interpolation—this is the disruption aspect. The second transfer results in the partial reinstatement of the interpolated activity, at the time of recall of the original activity. Since parts of the original activity are disrupted by the interpolated activity, and since parts of the

[3] A different view is apparently held, however, by Müller and Pilzecker (**23**), Heine (**12**) and Skaggs (**27**). Their view seems theoretically justifiable only for extremely short time intervals of, say, a few seconds between original and interpolated activity.

interpolated activity are initiated at relearning of the original activity, it necessarily follows that there must be some similarity between the original activity and the interpolated activity in order for retroactive inhibition to occur. The general relationship which probably exists between the degree of similarity and the amount of retroaction has been stated by Britt (**2**) as follows:

> "At the maximum of dissimilarity (in content, meaning, form, method, operation, environment, etc.) between the two activities, retroactive inhibition may occur. As the degree of similarity of one or all of these factors is relatively increased, the degree of retroactive inhibition also tends to increase. A certain point is eventually reached, however, after which increasing the degree of similarity results in more and more *actual identity* of the various factors (content, meaning, form, method, operation, environment, etc.); and from this point on, *i.e.*, identity of one or more factors, the amount of retroaction tends to decrease, until at the upper limit, actual identity of all the factors, there may be no inhibition at all but simply repetition."

These conclusions were based upon an examination of the experimental work of the following investigators, among others: Robinson (**25, 26**), Skaggs (**27**), Whitely (**32**), Cheng (**6**), Harden (**11**), McGeoch and McDonald (**18**), Dreis (**8**), Johnson (**13**), Peel (**24**), Gengerelli (**9**), Gibson and Gibson (**10**); and it is possible for the results of these experiments to be offered in support of the present extension of Webb's theory.

It should be particularly noted that the writer's suggestions do not require that the two types of activity, original and interpolated, must be similar as to contents alone. Webb (**31**) states that his "facts indicate that transfer is to a large extent a function of the particular relationship existing between the two activities." As Robinson (**25**) has pointed out, the original activity, the interpolated activity, and the recall of the original activity simply must have enough in common in content, or form, or process, or temporal contiguity "to insure the reinstatement of a part of one of the situations in intimate connection with another." Thus, the

transfer may be of any one or more of many factors: content, meaning, form, temporal contiguity, spatial contiguity, degree of organization, method employed in learning, instructions in learning, degree of learning, environmental conditions, amount of practice, etc. If transfer is to be accepted as the explanation of retroaction, it is highly probable that all of those factors which determine transfer of training also influence the amount of retroactive inhibition.

This does not mean, however, that every factor which results in greater positive transfer from one activity to another must necessarily result in greater retroactive inhibition. This would probably be true if the transfer were a transfer only of certain contents or elements of one activity to the other activity, for this transfer of parts of the original activity would result in disruption at the time of interpolation and would result in blocking at the time of reinstatement. Such a type of positive transfer would, ordinarily, mean a greater amount of retroactive inhibition.

However, this type of positive transfer, as has been pointed out above, is not the only kind. For example, there might be positive transfer from one activity to the other of the method employed in learning, of instructions in learning, of environmental conditions, etc. If the transfer were of this sort, and there were no transfer at all of the component parts of either problem, there would undoubtedly be a facilitating rather than an inhibiting effect. Greater familiarity with the method of learning, or better understanding of the instructions, or more ease in the environmental surroundings, etc., would aid the learner, first, in the interpolated activity, and, second, in the recall of the original learning problem. As a matter of fact, of course, it is not at all probable that there would ever be a situation in which there would be transfer of facilitating factors without transfer at the same time of some inhibiting factors; nor, *vice versa*, is it probable that in actual experience there would ever be transfer only of inhibiting factors without transfer of some facilitating factors as well. Therefore, as to whether a given situation in which there is positive transfer results in greater or less retroactive

inhibition than a situation in which there is not as much positive transfer must depend upon whether the items transferred in the two situations are more facilitating or more inhibiting in character. If a greater number of items transferred are actual component parts or elements of the learning problems themselves, and if these items are of greater strength than the other items, there might be a greater amount of retroaction. If, on the other hand, a greater number of items transferred are not component parts of the problems themselves but are simply related factors (such as methods used in learning, learning-instructions, environmental conditions), and if these factors are of greater strength than the elements of the problems themselves, there might be a lesser amount of retroaction.

Certain results of the study by Britt and Bunch (3) may be used as an illustration. These investigators wished to find out which of two associations of the same strength but of different ages is the more susceptible to retroactive inhibition. With 30 subjects in each group, the following experimental procedure was used:

Group	Learning	Interval	Conditions employed to reach level of complete mastery	Conditions of interpolated interval	Retention
I	Maze I	48 days	Maze I (Relearning)	Maze II	Maze I
II	Maze I	48 days	Maze I (Relearning)	20-min. rest	Maze I
III			Maze I (Learning)	Maze II	Maze I
IV			Maze I (Learning)	20-min. rest	Maze I

On the basis of the age of the associative connections, Groups I and II were referred to as the older age-group, and Groups III and IV as the younger age-group. Since the relative amount of retroactive inhibition in the older age-group was consistently far less than the relative amount of retroactive inhibition in the younger age-group, it was concluded that, "of two associations (maze habits) of equal strength but of unequal age, the younger is more susceptible to retroactive inhibition."

It should be noted, however, that in this experiment there was approximately *twice* as much positive transfer to the

interpolated problem with the older age-group as with the younger age-group.[4] The older group possibly became more familiar with the correct method of learning, learned to understand the meaning of the instructions better, were more at ease in the environmental conditions, etc., these various factors being transferred positively to the second learning problem. Anyhow, whatever factors were transferred, it is clear that more and/or stronger facilitating factors were transferred than inhibiting factors, for the older group was much less susceptible to retroactive inhibition than the younger group. This means that, in this experiment, the conditions of the greatest amount of positive transfer were also conditions of the least amount of retroactive inhibition. As has been indicated, however, this does not justify the conclusion that more positive transfer *always* means less retroaction. As to whether there is more or less retroaction depends upon whether there are more and/or stronger inhibiting, or more and/or stronger facilitating, items transferred.

With regard to the disruption hypothesis, Webb says that "the total transfer effect is the sum of both positive and negative elements." Also, "We may assume that the disrupting effect of negative transfer will be greater than in the case of positive transfer." Thus it is possible for the degree of disruption, and, consequently, the degree of retroactive inhibition, to be inversely related to degree of transfer. The results of the study by Britt and Bunch are in accord with this theory, for the younger age-group was much more susceptible to retroaction and yet showed much less positive transfer than the older age-group. Possibly the negative elements transferred were relatively greater in number and/or strength than the positive elements transferred for the younger group than for the older group.

With respect to the transfer effect, Webb says that it seems to be limited to the early stages in the acquisition of a habit. That is to say, transfer starts the subject at a more advanced stage of the habit, and has relatively little effect

[4] *Cf.* Britt (1).

upon its final development. On the other hand, in a retroactive experiment, dealing with remastery of a partially disintegrated habit, the habit is largely retained, and "the process of relearning is essentially similar to the final stages of development of a new habit. . . . Since transference exerts radically different effects upon the initial and the final stages of learning, one cannot assume that transfer must produce the same effects in our two experiments (these two were on transfer and retroaction); in fact one must assume that the effects are essentially different." The results of the study by Britt and Bunch fit in with this idea, as well as with the disruption theory. In fact, they illustrate one set of conditions under which the effects of transfer are 'essentially different.' The relation between age of association and the amount of transfer was such, in this experiment, that the older association showed great positive transfer to the interpolated activity but little retroaction, whereas the younger association showed little positive transfer to the interpolated activity but much retroaction.

The hypotheses of disruption and of transfer tend to supplement each other; either theory or both theories may be necessary for the explanation of any particular experiment involving retroactive inhibition. The amount of retroactive inhibition necessarily depends upon how many factors are being transferred, upon exactly which factors these are and how they compare in strength, and upon the time when the transfer takes place. It may be noted that some of the determining factors of retroactive inhibition, similarity, temporal position, degree of learning, various conditions present during learning, etc., have also been studied experimentally as determining factors of transfer of training, which indicates the closeness of relationship between the underlying causes of transfer and of retroactive inhibition.[5]

[5] Since the preparation of the above manuscript, McKinney and McGeoch (22) have reported experiments dealing with the delayed recalls of adjectives learned by the method of complete presentation, and followed either by no formal learning or by the interpolated learning of a list of synonyms. Under the work conditions they found overt transfer from the original to the interpolated list, and from the interpolated list to the original, although the latter was much greater. At least a fourth of the

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obtained retroactive inhibition could be ascribed to *overt* transfer; as to what to attribute the remaining three-fourths, they suggested: "The most probable hypothesis is that at least a major part of the inhibition from interpolated learning not ascribable directly to overt transfer is to be attributed to interference of the kind called negative transfer."

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SOME CATEGORIES IN PSYCHOLOGY

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To define psychology adequately is to limit tasks, state problems, describe methods, differentiate data and outline causation. We wish to touch briefly upon the nature of tasks, data and causation in order to indicate what appears to us to be a fruitful approach to psychology. We are unable to find a place in psychology for the concept of stimulus as a physical or chemical agent to which an individual responds in a psychological manner. As a result of discarding this conception of the stimulus, we are also unable to find a place for sensation and image when considered as simple mental elements. Into the discard with these alleged bits of mind, there must necessarily pass the notion of the mind as an existence, agency, entity or force. We suggest that psychology has a rightful and wholly distinctive place if it will but lay claim to it. Psychology would do well to recognize that its problems, in common with other sciences, are essentially functional. Nothing can be scientifically known about a thing except when considered in terms of the relations which it sustains toward some other thing. Man can accordingly be studied only in terms of particular situations, by what is done in them. What a thing does, however, can be known only by the nature of its products. The chemist, for instance, recognizes an acid by what it does to a substance or an object; the geneticist studies heredity in terms of the nature and number of products; the physicist determines the properties of electricity by the changes it produces in some thing—a piece of apparatus or a human being. Psychology can find its proper place among other sciences by directly recognizing that their problems are in part its problems. Thus the scientist who attempts to develop a new dye will quite readily grant that the particular hue of the substance

which he seeks is not chemical in the sense that it can be detected by any known chemical methods. He is willing to grant that it is psychological. He definitely describes the hue as being one characteristic of a particular chemical substance. The traditional psychologist, however, seemingly refuses to have anything to do with some common stuff. The color which the chemist seeks cannot be a property of the substance, for it is alleged to be a *sensation*—a mental thing—a bit of his mind. But when he is pressed to localize this *mind*, to give it a habitation so that it may thereby be *naturalized*, he finds himself at a loss. And while he searches, psychology, we suggest, suffers.[1]

Less confusion, we are sure, is most certain to follow a shift away from the sterile distinction commonly drawn, for example, between sensation and perception where sensation is regarded as being *simpler* than, and *qualitatively* different from perception, and where the latter is considered to be composed of a determinable number of the former. In their text, for example, Warren and Carmichael write that "our perceptions are *more* like the object than the sensations which compose the experience would lead us to suspect." [2] Nothing is directly added to our understanding of psychology by maintaining, for instance, that in every observation there is some sensation which is first aroused and is *fundamental* and *non-derivative* and that some perception is *later* aroused and *is derived* from this sensation. It is from such a position that statements of the following sort find their way into our textbooks: "On touching something in the dark," Woodworth states, "you may feel it as one thing and another till some perception is aroused that fits the known situation and satisfies you. Such shifts of perception, while sensation remains virtually unchanged, can be frequently observed if one is on the watch." [3]

If the understanding individual will consider the signifi-

[1] In his 'New field of psychology,' M. Bentley provides an illuminating departure from this position.

[2] H. C. Warren and L. Carmichael, Elements of human psychology, Boston: Houghton Mifflin, 1930, p. 144.

[3] R. S. Woodworth, Psychology, New York: Henry Holt, 1929, p. 421.

cance of this illustration, he will most likely question the scientific desirability of ever raising this distinction. He will surely discover that this is not, for instance, a form of psychological analysis through observation. He will grant, of course, that he may feel an *object* in complete darkness. Moreover, he may observe it as being *round* or *square*, *hard* or *soft*, *large* or *small*, *smooth* or *rough*. These are observable properties of the perceived object. They are not sensations in his mind. Science has not reduced observed objects to mental elements. These various properties may be repeatedly observed upon subsequent occasions. The smoothness, for instance, is to be observed whenever an individual touches the object under particular conditions. Perhaps he may also say: "It is an apple," or "It is a rubber-ball," or "It is a wooden block." When an object is observed in this latter way, the individual has probably gone to the greatest length possible through perception. He has *classified* the thing touched, that is, a particular class property has emerged. At the same time he has arrived at the highest use to which perception may be put in science—where an individual may become so thoroughly familiarized with an object (group of objects) that an instantaneous glance is sufficient to enable him to place a thing in its proper relationship or classification with respect to other things. A difference in color, for example, may mean a difference in species or race. In the same way, of course, the non-scientist may also instantly perceive a thing (a book or a human being) as belonging to a particular group or class of objects. Class property belongs to the whole object. In this respect, it is like any ordinary case of familiarity. We shall do well if we are constantly guided in our thinking by this fact: there is always an *object* which is observed in terms of one or more of its possible psychological properties or characteristics. But there is *never* just a characteristic—a kind of Alice-in-Wonderland-grin-without-the-cat characteristic.

We definitely assume, scientifically and pedagogically speaking, that psychology would be greatly clarified by completely abandoning the naïve notion of the stimulus as

something to which man directly responds in life situations. This use unquestionably involves a gross confusion of sciences. Man does respond chemically, physiologically and physically. He may thus change through exposure to water by developing a cramp in his stomach and a cold in his head; or he may shiver from a lowered bodily temperature and float because of his fat tissues. These are physical, chemical and physiological responses. To have a consistent psychology, however, man must necessarily *observe* that to which he responds. The writer has repeatedly found it to be a bit difficult after a course is well under way to point out to his students exactly what specific stimuli exist in a large number of cases such as hallucination, dizziness, imagination, memory, thirst, delusion, hunger for sweets or relishes, and distance. Of distance, Titchener, for example, remarks that "we seem, quite immediately and directly, to see distance." The writer has also been surprised to have students report that they perceive physical light. They do not consider that environmental objects are merely seen differently (have different properties) under different degrees of illumination. They fail to recognize that light constitutes one form of causation. It stands along with other forms of physical, chemical and biological energy. Ordinarily, man does not observe energy. The physicist, for instance, does not pretend to observe the physical properties of energy. He conducts his studies by instrumentation which permits him to remain experimentally at a physical level. Why some psychologists should persistently remain at a naïve level is a bit puzzling. Of those who sin here, the behaviorist is most guilty. They are, however, no different from those who find significance in the 'stimulus error.'

When a normal individual opens his eyes upon a crowded street, millions of light waves streaming from literally thousands of different sources of energy may strike his retinas. He can do nothing about it; and, as a matter of fact, he directly knows nothing about this *extensive* bombardment. Yet, despite all these unlike stimuli which necessarily act in a purely physical and mechanical manner upon his open eyes,

he may not observe any object. We can say, therefore, that man's different *sense-organs* mechanically select certain stimuli, because they are, very broadly speaking, biologically tuned to different forms of physical energy. That is, they operate differently with respect to different forms of physical and chemical energy. In somewhat the same way, we can say that an ordinary sieve selects certain objects and at the same time rejects other objects which are placed in it because it has been definitely prepared previously to select certain objects. The stimulus, we suggest, is important in being *one possible cause* of the activities of living organisms, but it unquestionably operates below the *meaningful* level of function. In experimental studies of threshold limits, for instance, some one property (length, color, sound, weight) of an environmental object may be observed. The observer merely reports when a particular characteristic appears, disappears, is different or is equal.

We accordingly suggest that the psychologist at all times observes merely the several properties of objects (either human or non-human—himself or some other thing). He always observes an object in a situation in terms of its various characteristics. And these particular properties are definitely psychological. He observes, among many properties, those of color, size, distance, pain, sourness, roughness, familiarity and movement. Consider a simple illustration. Beebe,[4] for instance, reports that he paused one day near the sea bottom to observe a huge turtle which floated motionless a few feet above him. Suddenly, as he says, he was as surprised as if on land he should see the sun begin to bob back and forth. The huge turtle as well as thousands of other things were all bobbing back and forth. Beebe had at this moment placed his feet upon the bottom. By *lifting* himself a few inches away from the bottom the objects became completely motionless. Physically speaking, they were always in motion; psychologically speaking, they were motionless part of the time and moving part of the time. The movement of an object, as observed by a human being, is a psychological

[4] C. W. Beebe, Arcturus adventure, New York: G. P. Putnam, 1926, p. 181.

property. We can therefore say that the psychologist never observes movement and other psychological characteristics in the absence of an object. In short, they *are* the object or situation when considered at a psychological level, just as chemical properties may be said, scientifically speaking, to constitute any known chemical substance. These patterns of object properties are functional products of man; they never occur in his absence. There is, for example, no known chemical test to determine the sweetness of a chemical substance. If there were such a method we would be forced to grant that sweetness is a chemical property. It is, however, a psychological property—one which is always dependent *in part* upon a functioning organism. That is, sweetness is an observable property of some thing which emerges under a total set of causal conditions in which both a form of chemical energy and a functioning organism are necessary factors. It is as much a matter of *chemistry* as of *psychology*. There is, moreover, another important aspect of this psychological property. It is a characteristic of the *whole* chemical compound. That is, the slightest change in the C, H or O sides (*e.g.*, sugar) will completely destroy the property. It is, therefore, an all-or-none characteristic.

These observed properties of environmental objects are meaningful characteristics. They may be perceived, imagined, remembered or understood. In every case, the brain functions and directly produces an object or situation which has definitely describable characteristics such as familiarity—size, shape, color or danger. A remembered object, for example, is certainly not mental. At this moment, I remember one who has long been dead, but that does not reduce him to a mental status. I remember a companion who was killed while hunting with me. The properties of size, weight, height, color which characterize him belong to a particular individual. The properties now and then are alike; the functional mode merely differs. Then he was perceived; now he is remembered. This shift in mode does not make him into a mental object. To hold that it does involves among other things a gross confusion of physics and psychology.

After brain function, there comes meaning—not sensation. Meaning is an *observable* property or characteristic of a thing. It may be perceived; or it may be understood. In his new 'Fundamentals' Pillsbury points out that "We may *assume* the existence of sensations and of recalled images or centrally aroused sensations and then try to develop a notion of the way meanings are added to them. We are *aware of meanings* first and then build up theories of the presence of sensations to explain how the meaningful experience originates." [5] It is against such a sterile program that we raise our voice.

Speaking strictly in terms of observation, nothing is to be found between brain function and a perceived thing. No individual has effected a separation of (1) observed objects on the one hand, and (2) mental forces, parts, elements on the other hand. No psychologist has ever directly reduced meaningful things in a situation to non-meaningful entities in the mind. One certainly does not analyze an observed object and thereby secure mental elements. We accordingly find little significance in questions of this sort: When one remembers an object, is the idea that comes to *consciousness* the same idea that he had when he thought of it on former occasions, or is it a different one? We suggest that no one ever *revives* any bit of his mind. What one does is simply to produce an observed situation similar to that which he earlier perceived. It is, psychologically speaking, just meaningful patterns of characteristics or properties which again appear. Memorial objects are actually no more mental than perceptual objects are. Both are observable; both are products of a brain functioning in different situations.

Many studies made in various fields could be cited in confirmation of our position. Experiments on thinking—imageless thought—illustrate quite clearly our point. Men have repeatedly reported that the solution to a thought situation appears in the absence of any mental aids such as sensations or images. The subjects, for instance, faced a situation composed of one set of observable properties. When

[5] W. B. Pillsbury, Fundamentals of psychology, New York: MacMillan, 1934, p. 390.

the solution appeared, a new characteristic emerged. In emotional situations, again, no one has ever reported that sensations and images first appear and then meaning follows. One observes a situation in terms of particular meaningful properties and this observation constitutes an emotion. For example, we agree with Cannon that the brain functions and an emotional situation appears. It is an observed situation which may be partly or wholly independent of somatic changes. The various experimental studies on possible physiological changes accompanying an emotional situation have clearly revealed the part which such changes play. Moreover, the failure of Sherman's trained observers to identify emotional behavior in the absence of knowledge of the emotional situation definitely suggests the importance which the situational meaning plays in emotional functioning.

The kind of approach which we suggest here gives psychology a legitimate place among the other sciences. Its task is to deal descriptively within limits with certain properties of the *same* objects that concern the other sciences. Psychology, descriptively regarded, is concerned neither with the observation of an individual's mind nor with his glandular and muscular changes. The former has no scientific existence and the latter are physiological. The use of physical, chemical and physiological methods in a psychological laboratory does not necessarily make the derived data psychological. The employment, for example, of a string galvanometer or a radio amplifier in a psychological laboratory does not give the data obtainable a psychological nature. To deny the significance of method, in this respect, is to strike at the very heart of science. Psychological methods necessarily proceed at a *meaningful* level. Tests, rating scales, questionnaires, verbal descriptions and descriptive reports of behavior in meaningful situations illustrate some of its methods. Combined with other methods, these definitely contribute to man's understanding of his organic functioning. When it turns to *explanation*, psychology draws heavily upon the methods of other sciences. It is accordingly impossible to write an adequate psychology without involving physiology and neurology.

Our way of approach, moreover, provides an adequate integration. It does not necessitate changing horses several times while crossing the field of psychology. We insist that psychology should not start at the submeaningful level of stimulus and sense-organs and attempt to build upward in the hope of reaching a meaningful level, but that it should actually start at a meaningful level and proceed onward from there. Our way, we suggest, does not lead from the meaningless to the meaningful. Observation is *always* meaningful because one simply observes some one or more properties of an object. Moreover, the brain functions at all times in the young child just as in the adult to produce meaningful characteristics. Many of these characteristics seemingly persist unchanged throughout many years. The young child, for example, directly observes the bitterness of quinine. It spews it out. As far as we know, there is not the least evidence indicating that this observed bitterness gradually increases with the years. What the child observes in the beginning, it also observes at the end. We do not assume that the child (or the animal) observes 'sensations' in such cases, when trained adult observers allegedly do not do so. We have every reason to assume that if the child, for instance, never saw nor handled the quinine, it would *always* be descriptively reported as 'that bitter stuff.' What is true of the property of bitterness should be regarded as holding similarly for many other psychological properties. Many emerge early in life and persist unchanged as long as the causal factors persist.

Our approach also integrates the normal and the abnormal. Thurstone has pointed out that psychology suddenly shifts its ground in passing from a consideration of the normal to the abnormal. The normal individual allegedly responds to stimulus, but the abnormal person finds no stimulus to which to respond. If we will simply recognize that the abnormal individual always produces meaningful situations in which, for example, fearful characteristics may appear and to which he may respond by screaming in terror or by killing in rage, we can thus relate such functioning to that of the

normal individual. He, too, observes meaningful features of situations and responds directly in terms of them. It may be a fearful situation in which he may run, crawl, roll, dig a hole, hide under objects or lie quietly. The physical stimulus, speaking in terms of its physical nature, cannot possibly produce such diversity of responses unless such a naïve attitude is taken that a fearful situation—a burglar with a gun in his hand—is called the stimulus. However, to speak of stimulus in this way is to place psychology forever beyond the sciences of physics and chemistry.

If we will assume that psychology is concerned with brain function, not mind function, we can speak a common language with other sciences. We can readily accept the categories of physics, chemistry, neurology and physiology as explanatory agencies. We will understand that the brain *continuously* functions at some level during every moment of an individual's life. It is never idle. When acted upon by energy, changes occur. The level of function may be either raised or lowered. Stimulation, strictly speaking, may either *decrease* or *increase* the level of brain functioning. If we assume that brain function is directly accompanied by the emergence of meaningful objects and situations, we can understand in part how the stimulation of an exposed brain by a mild electric current may result in the immediate observation of an object, either moving or stationary, of particular color, size, shape, distance and familiarity. We can also understand that an ingested drug is not directly observed but that, as one result, environmental objects possessing many different characteristics are directly observed. Since we do not go beyond the known laws governing energy exchange, we can also understand how various metabolic changes in the somatic tissues may well affect the psychological properties of observed situations. We do not have to look for some strange way in which the body affects the mind or the mind affects the body. We deal always with a functional unit in an environmental situation.

Our approach serves to resolve the mind-body problem. As McGeoch has recently pointed out, every sensible approach

to psychology must consider this problem. We find that there is more in life situations than physics, chemistry and physiology. Nor do we discover the slightest justification for alleged descriptions of observed objects and situations in terms of subvocal and implicit movements. Psychology is certainly more than quivering muscles and secreting glands. This problem of mind and body is always with us. In a recent book, for example, Robinson again raises this old question. "One is likely to assume," he writes, "that what flows up the incoming nerve is the *sensation* (of weight). If one is careful he will not make this assumption, because he will realize that a sensation would not be discernible no matter how carefully the structure of the nerves were examined. No one has ever found a sensation travelling over a nerve nor does one expect to find one. It may be assumed that the sensation is in the brain and that proper methods of examination would reveal it there. But the brain is merely a way station of the neural arc. It is made of nerve cells of the same essential character as those that conduct into the brain and out from the brain, and there is no reason to think that we should find in the brain anything more than is the spread of that type of electro-chemical disturbance found in active nerves in any other part of the system. *Exactly where is the sensation, may be a fair question, but it is not one which the conception of the neural arc answers. It shows what kind of a conducting system must be intact and functioning in order that such a fact as a sensation of weight or color or tone may occur, but it indicates nothing about the spatial location of the sensation itself. It may be unfortunate that it does not do so, but if it did, it would be a conjecture*" (italics ours).[6]

We are unable to see the least possibility of building a science of psychology upon such foundations. To attempt to describe an elusive something which cannot possibly be localized might possibly constitute an interesting pastime for a philosopher but it should scarcely be expected to attract a scientist. We must admit that a psychological property is

[6] E. S. Robinson, Man as psychology sees him, New York: Macmillan, 1932, p. 264.

not in the sense-organs, nor is it in the nervous pathways which lead to the brain nor is it in the brain itself. Why not admit that a psychological characteristic, such as the bitterness of quinine, is an inseparable feature of an object? Under definite conditions it can be observed, described, compared, classified and understood. Acidity, for instance, is a functional property of a chemical substance. It can be known only in terms of its products. Perception is a functional property of a human organism. It can be known only in terms of its products. Last year, for instance, there was a piece of court litigation which had both wide legal and economic significance. It concerned the question of taxability of electricity. To the writer's excusable scientific chagrin, he read that of three properties of electricity named from the witness chair, two were wholly psychological. Langmuir, director of research for General Electric, testified that the weight of the power line increased when a current was present; Compton testified that electricity could be felt and Carlson stated that it could be tasted. When psychology finally decides to deal with meaningful (describable) properties of objects, as do other sciences, it should find its rightful place in the scheme of affairs. It has, we suggest, an incontestable claim if it will but stake it out and work it properly.

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THE METHOD OF COMMON POINTS OF MASTERY AS A TECHNIQUE IN HUMAN LEARNING EXPERIMENTATION [1]

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The comparison of groups or individuals with regard to learning ability is frequently rendered peculiarly difficult by virtue of the fact that initial ability varies between the groups or individuals. Let us assume, for example, that one subject starts at a level of ability indicated by a score of 40 units of work per trial and improves over the period of the experiment to a score of 60 units. Another subject starts at 30 units and improves in the same number of trials to a score of 50. Both subjects have shown an absolute gain of 20 points.

If we employ one traditional definition of learning ability and express improvement as a percentage of the initial score, we must assign the first subject a rating of 50 per cent and the second a rating of $66\frac{2}{3}$ per cent. If, however, we employ absolute gain as a measure of learning ability, we must conclude that the subjects are equal. Neither conclusion is warranted as neither definition is valid.

The percentage method is invalid because it assumes that the arbitrary zero point of the test coincides exactly with the absolute zero point of the ability being considered. This assumption is unwarranted. To say that improvement from a score of 40 to one of 60 represents a gain of 50 per cent is as misleading as the statement that 110 degrees Centigrade is 10 per cent hotter than 100 degrees. However, in the case of temperatures the absolute zero point is known and can be used in the computation of the percentage of change in temperatures. If the absolute zero point of ability were known in the case of a learning experiment or any other mental test, we should be much nearer our goal.

[1] Read at the Midwestern Psychological Association Meetings at Lawrence, Kansas, May 10, 1935.

Psychology must also solve the problem of units before learning experimentation can become quantitatively exact. In the original example gain was expressed as the difference between the initial and final performance of the subject. This method of defining learning ability is free from the objection raised against percentage definitions of learning ability but is, unfortunately, rendered invalid by the fact that units at different levels of performance probably have different values. A raw score difference of ten points at one end of the scale is not necessarily equal to a raw score difference of ten points at the other end of the scale. The elimination of the first error in a learning experiment does not have the same signification as a measure of ability as the elimination of the last error.

By way of summarizing the difficulties in using traditional definitions of learning ability, we repeat that the absence of an absolute zero point and the lack of true units render invalid any definition using a ratio or percentage, while the absolute gain definition is valid only if initial ability is the same for all subjects. There is a method based on the establishment of common points of mastery which will surmount these difficulties. One use of one variant of this method is reported by Thorndike.[2] Ruch[3] has reported other uses of this method.

The method of common points of mastery has two variants. The initial steps in treating the raw data are the same for both.

The learning curves based on the trial scores for each subject are smoothed by a running average covering enough points to eliminate serious chance fluctuations in the scores. After all of the individual records are smoothed in this fashion an initial common point of mastery is selected. That is to say, a particular level of performance common to the early trials of all subjects is determined by inspection. From this point on the procedure varies according to which of the two common point methods is used.

In Method A a second or final common point of mastery is

[2] E. L. Thorndike, Adult learning, New York: Macmillan, 1928.

[3] F. L. Ruch, Adult learning, *Psychol. Bull.*, 1933, 30, 387–414.

selected. This point must also be common to the curves of all subjects. Learning ability is then defined as the number of trials or the number of errors or the amount of time required to improve from the initial to the final common point of mastery. It should be noticed that this definition of learning ability is free from the objections raised against the two traditional definitions. All subjects pass through the same range of performance. They all start at the same point. The only assumption which need be made here is that ten trials represent more effort than nine trials.

In Method B the initial common point of mastery is established in the manner just described and learning ability is defined as the amount of improvement in the first segment of trials beyond this common point. The length of the segment employed will be constant for all subjects and will depend upon the statistical reliability of the data. It will be seen that with Method B all subjects start at the same point of mastery and the difficulties encountered when initial performance varies from subject to subject or from group to group are thus obviated.

Two problems of major significance in the psychology of learning have been attacked with this methodology. The results in both instances are gratifying if they are to be regarded as typical of all results to follow.

The first problem studied was that of the relationship between initial learning ability and physiological limit. A search of the literature revealed three long-time training experiments for which the individual subject's records by trial were available. These groups of published data have been analyzed by the common point method.

The first of these groups was from an experiment conducted by Wells [4] in which ten subjects practiced addition five minutes per day for thirty days consecutively except for Sundays. Of the original ten subjects only eight could be used as the other two did not show sufficient overlapping. This number is too small to have any real significance but

[4] F. L. Wells, The relation of practice to individual differences, *Amer. J. Psychol.*, 1912, 23, 75–88.

since the results of this analysis are consistent with those based on larger groups, they are included in this report.

The second group of data was composed of the individual records of fifteen subjects who practiced the three-hole coördination test in connection with Hollingworth's [5] experiment on the effects of caffeine. Each subject made 500 touches in a practice period and there were 26 of the practice periods. The scores used were the time required for 500 touches. These data are given in the appendix of Thorndike's 'Adult Learning.' It was necessary to omit one subject who did not show a common point.

The third and largest group of subjects was those of Chapman's [6] experiment on learning to typewrite. The 19 subjects who completed at least 154 hours of practice were included in this analysis. The analysis of the Chapman data is especially interesting because of the practical importance of accurate prediction of final ability in typing. Moreover, the number of cases was greater than in the other two experiments and the total amount of practice was greater. It must be pointed out that the subjects in the Chapman experiment had not yet reached their physiological limits but had probably reached a point beyond which no further criss-crossing of the learning curves would occur. The subjects were tested every second hour by means of five-minute tests. These test scores are the data analyzed in this study. Since certain subjects missed some of the tests, it was necessary to interpolate. This was done before the curves were smoothed.

All curves were smoothed by a running average covering ten points. Thus each point on the curves after smoothing represented about 50 minutes of testing in the case of each of the three groups. With tests of this length statistical reliability may be assumed.

Table I shows the degrees of correlation between final ability and initial ability as variously defined. It will be noticed that the common point method gives higher pre-

[5] H. L. Hollingworth, The influence of caffein on mental and motor efficiency, *Arch. Psychol.*, 1912, No. 22.

[6] J. C. Chapman, The learning curve in typewriting, *J. Appl. Psychol.*, 1919, 3, 252–268.

dictions throughout. The common point method predicts final ability even better than the traditional method involving twice as many trials.

TABLE I

SHOWING THE CORRELATIONS BETWEEN FINAL ABILITY AND THREE MEASURES OF INITIAL GAINS FOR THREE KINDS OF MATERIAL

Data from	Common point method	Traditional based on 1st half of trials	Traditional based on all trials
	'r'	'r'	'r'
Wells	.99	.59	.81
Hollingworth	.80	.45	.38
Chapman	.63	.41	.39

Preliminary investigations indicate that the method of common points of mastery has considerable promise in the study of the intercorrelations between the abilities to learn different tasks.

In Hollingworth's original experiment the sixteen subjects learned three tasks: three-hole coördination, opposites and calculation. The individual records are given in Thorndike's 'Adult Learning' for the first two of these tasks. In the opposites test performance was scored as the amount of time required to name 250 opposites. The data were smoothed as before by means of a ten-point running average.

Table II gives the intercorrelations between learning ability in the two tasks. It will be seen that the common point method yields higher correlations than the traditional methods.

TABLE II

SHOWING THE INTERCORRELATIONS OF LEARNING ABILITIES FOR MOTOR COÖRDINATION AND OPPOSITES

Method	'r'
Common point	.59
Difference between 1st and 4th blocks of 5 trials	−.14
Difference between 1st and 2nd blocks of 5 trials	−.17
Difference between 3rd and 4th blocks of 5 trials	−.11

Some explanation of the higher correlations obtained with the common point method as compared with the traditional method is in order. To make the explanation simple, let us assume that there *is* a correlation between innate learning

ability and physiological limit or between innate ability to learn two different tasks. In the typical learning experiment the subjects are far from unpracticed at the moment the conditions of a formal learning experiment are imposed upon them. They have already had some opportunity to practice the skill in question. How much haphazard practice they have had is not on the record. What we know definitely is that subjects show individual differences in performance on the very first trial of the formal experiment. One subject might have a high initial status because he has had much unrecorded prior practice though he may be only average in native learning capacity. Another subject might make a low initial score because he is average in native capacity and has had but a small amount of prior practice. A third subject might stand high on the first trial by virtue of great native capacity even though he has had little unrecorded prior practice. These are just a few of the combinations of prior practice, native capacity, and initial performance. Add to this motley of variable unknowns the further fact that absolute gains have different meanings in terms of ability at different ranges of the scale and it would appear that in the traditional method we are dealing with enough attenuating and distorting factors to conceal a fairly sizeable correlation. The method of common points eliminates variable initial ability and allows gain to be determined systematically by native capacity beyond that point.

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LOTZE'S INFLUENCE ON THE PSYCHOLOGY OF WILLIAM JAMES

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It is said nowadays that every important new idea suffers a curious fate: at first it is hailed as original but untrue; then, when its validity becomes generally recognized, numerous lesser lights assure us that, while true, it is wholly unoriginal, that it has been said long ago and better by, say, Plato, Spinoza, Kant or Hegel. The necessity of being both fair and sharply discriminating makes the estimation of the extent to which one thinker has influenced another a hazardous and problematical undertaking. Should 'influenced' mean that a given author adopts certain of the ideas of a contemporary or predecessor and fits them into the frame-work of his own system? If so, it would be easy to reduce the system of William James, for example, to its components, by pointing out that this comes from Wundt, that from Renouvier, this again from Hodgson, another idea from Stumpf, and that again from Lotze, etc., until all his principal ideas had been parceled out among his 'sources.' Such a procedure would fail grotesquely to do justice to the conspicuous originality of William James.

Should 'influence,' then, imply a kind of Socratic midwifery in the process of which the 'influencer' serves to develop and fortify ideas native to the 'influencee'? Certainly in the case of James, and presumably for any other significant figure, this is the safer assumption to make. It is primarily in this sense of the word that Lotze's influence on James is to be considered here.

There are those who are skeptical of the value of even the most circumspect and sophisticated studies of the historical sources of ideas and systems. Particularly is this the case in a field that is new and undergoing rapid change; where there

is a disposition to work in the present and hope for the future—and forget the past. In the eighties and nineties James commented frequently on the extreme youth of psychology. It was, he insisted, still in its swaddling clothes. In our day, however, there are unmistakable signs of approaching maturity, the most conspicuous of which is the interest, on the part of psychologists, in the history of their subject; witness the appearance of at least three histories of psychology in the past six years in America. It is no longer necessary to be on the defensive in presenting a study in the history of psychology.

An apologetic would be particularly superfluous in connection with an examination of the sources from which James received the most stimulation. For he was one of the first formative influences in American psychology up to the War, and he remains a figure to be reckoned with to the present day. Furthermore, James was for his generation the chief vehicle for the transmission of foreign ideas in psychology to the American public. What he read, to whom he acknowledged his indebtedness, and whom he found stimulating and in what respects, is not only of biographical interest, but may shed some light on influential conceptions in recent and current American psychology.

Not the least among the thinkers who exercised a decisive influence on James' development, and one to whom James pays special homage in the Preface to his 'Principles,' [1] is Rudolph Hermann Lotze. While Lotze's name appears frequently on James' pages, the exact spheres and extent of his influence is exceedingly difficult to determine. James' wide reading, his susceptibility to ideas from many quarters, his remarkable capacity for assimilating such ideas and presenting them with characteristic force and vigor, his singular lack of interest in any but the recent history of psychology,

[1] ". . . I cannot resist the temptation at the end of my first literary venture to record my gratitude for the inspiration I have got from the writings of J. S. Mill, Lotze, Renouvier, Hodgson, and Wundt. . . .", Principles of psychology, New York: Henry Holt, 1890, 1, p. vii. James wrote G. Stanley Hall in 1879: ". . . it gratified and at the same time disgusted me to see how many choice pages of my everlasting psychology had been anticipated by Lotze." R. B. Perry, The thought and character of William James, Boston: Little Brown, 1935, 11, p. 16.

and his admittedly unsystematic citation, documentation and bibliography—the latter accounts for his chivalrous acknowledgments in some cases and the unwitting neglect in others—, these factors mightily complicate the task of sifting out any one thinker's influence on James. Additional obstacles are offered by the eclectic, dialectical, and ofttimes indecisive nature of Lotze's thought, the absence of a distinct and unambiguous Lotzean doctrine or school, and the voluminousness of his writings. Finally, add the fact that the spiritual and intellectual pattern of an age causes certain general ideas and standpoints to be in the air, so to speak, ready to be snatched up and formulated by independent investigators, and one can readily appreciate the necessity of eschewing all dogmatism and finality from this investigation.[2]

I

Lotze's ideas became a factor in James' thinking as early as 1867; in that year, while abroad for his health and for study, James came into possession of a copy of Lotze's 'Medicinische psychologie'[3] (1852), and to all appearances he undertook a very thorough study of this work at once.[4] As James was just beginning to manifest a strong interest in psychology at this time,[5] Lotze became one of his earliest teachers. The 'Mikro-

[2] The author was especially fortunate in securing from Henry James, Jr., and from Prof. R. B. Perry, permission to examine the material in the library of William James, as well as a mass of unpublished notes, manuscripts, letters and diaries of James, all housed in the Treasure Room of Widener Library, Harvard University. Special acknowledgment is due Prof. Perry for several valuable leads pertaining to this work. Unfortunately, the latter's truly monumental work on James (cited above) came off the press too late to be of extended benefit to this study.

[3] On the flyleaf of the James copy we find in James' hand, "Berlin, Jany. 1867," and lower, "Dresden, July, 1868." The former date does not agree exactly with the facts of James' German visit as recounted in Henry James' 'Letters of William James,' Boston: Atlantic Monthly Press, 1920, 1, pp. 84–139. According to this account William was not in Berlin until the autumn of 1867. However, the exact date is of little moment here.

[4] His copy contains copious analytical notes, marginalia, underscorings, cross-references, inserts, etc. This is also true of most of the other works of Lotze in the James Library.

[5] In a letter to Thomas Ward in 1867 ('Letters,' I, pp. 118–119), James writes: "I have blocked out some reading in physiology and psychology which I hope to execute this winter. . . . I am going to study what is already known, and perhaps may be able to do some work at it."

kosmus,' which among many other things contains a popular restatement of the 'Psychologie,' James secured in the second (1869) German edition in 1876.[6] Upon the appearance of the English translation of this work in 1885, James immediately employed it as a text in his classes in 'Philosophy 3.' He secured Lotze's large 'Logik' (1874) and 'Metaphysik' (1879) immediately upon publication and studied both exhaustively. The latter is of especial importance in this connection because of its Book III on 'Rational psychology.' All these works had been read by James before the appearance of his 'Principles' in 1890, and all but the 'Metaphysik' before 1878, the year he contracted with his publisher for the forthcoming treatise. There can be no doubt that these works made a deep impression on James' alert and receptive mind during its most formative period. The general respect in which he held Lotze is manifest not only in the frequent mention of his name and references to his works, but in the epithets, 'subtle, fertile, acute, able' which he applies to him.[7]

This tedious array of dates makes it apparent that Lotze was certainly in a *position* to be a major factor in James' development as a psychologist. Before proceeding with a more detailed examination of the special provinces in which Lotzean influence is manifest, it is instructive to note the remarkable biographical parallels and intellectual and spiritual likenesses in James and Lotze. Both received their early training chiefly in physiology and medicine, devoted themselves to psychology during the middle period of their careers, and to philosophy in the last.[8] Because of their training in the natural sciences both exhibit a strong empirical and posi-

[6] From the date on the flyleaf in James' hand. This, by the way, corrects a minor error in Carl Stumpf's admirable little essay, 'William James—nach seinen Briefen' (Berlin, 1928), wherein the author states (p. 28): "Dabei scheint er von Lotze (ich kann es jetzt nicht mehr genauer kontrollieren) nur die 'Medicinische Psychologie' gekannt zu haben, nicht den 'Mikrokosmus' . . . ."

[7] Stumpf says (*op. cit.*, p. 28): "In James' 'Principles of psychology' findet man Lotze und Brentano, die grossen Meister der analytischen Psychologie und der Selbstbeobachtung . . . immer mit Zustimmung und Bewunderung erwähnt."

[8] After the abridgment of the 'Principles' in his 'Psychology: Briefer course,' New York: Henry Holt, 1893 (the two books were nicknamed 'James' and 'Jimmy' in the James family), James rapidly lost interest in general psychology and became irritated at being referred to as a 'Psychologist,' 'Letters,' II, pp. 2–3.

tivistic bent; [9] however, in Lotze this tendency often capitulated to that all-engulfing unity which he had inherited from the idealistic *Deutsche Bewegung*, while James could find in the facts no warrant for going beyond an emphatic pluralism.

Both James and Lotze stress the dynamic and functional elements in their interpretation of mental life. Neither had the true temper of an experimentalist or 'laboratory man,' nor did either make any outstanding contributions to this field.[10] Both were peculiarly possessed of an insatiable *personal* need to grasp the meaning of life and existence; hence the doctrines of both authors are ultimately motivated by a sensitive regard for æsthetic, moral, and religious values, and both finally affirm the 'priority of the practical reason,' though employing different arguments. And for both, the two *Urstellungen* which Nietzsche described as the Apollinian and the Dionysian [11] remain fundamentally unreconciled, through unwillingness to sacrifice the *Gemüt* on the alter of mechanistic science and philosophical materialism.

These facts make it apparent that Lotze was not only in a position to influence James, but that the American psychologist must have recognized in his German predecessor a kindred spirit, and lent him a willing and sympathetic ear. Lotze's formulations and criticisms served to bring many of the groping ideas of the youthful James to a sharp focus and to reinforce them with the weight of established authority.

[9] One of the things that James most admired about Lotze was his respect for the teeming universe of fact. "A very few men can both quarry materials with the barbarians and shape them with the philosophers. Lotze in our generation is one of these. . . .", Unsigned review of Ladd's 'Physiological psychology,' *Nation*, 1887 44, 473.

[10] Lotze, of course, antedated the real morning of experimental psychology, but shows no disposition to go beyond simple introspection, in which, however, he, like James, was peculiarly gifted. James' few experimental attempts impress one as conscientious but uninspired. See, for example, 'The sense of dizziness in deaf-mutes', *Am. J. Otol.*, 1882, 4, 239–254. Later he even conceived something of a contempt for experimentalism. *Cf.*, the passages in 'Letters,' II, 2–10; and 'Principles,' I, 192–193.

[11] *Cf.*, W. McDougall's interesting attempt to clarify 'The present chaos in psychology,' by applying Nietzsche's celebrated distinction, *J. Phil. Studies*, 1930, 5, 353–362.

And this is practically the only form in which the original and richly endowed spirit of William James could be influenced.

II

In passing now to the examination of a number of special aspects of James' psychology in which the influence of Lotze is manifest, we are confronted with the problem of the method and aims of psychology. Consider, first, the qualified and extremely cautious conception of Lotze.

He carefully counts the cost of making psychology a pure natural science, as the prevailing 'Modephrase' would have it. "It is our intention," he states, "to develop a conception of soul-life, which, while it completely satisfies the requirements of the natural science viewpoint, will nonetheless leave the way open for the attachment of those moral and religious reflections, whose right to consideration is unjustly challenged by the [scientific] fanaticism of our times." [12] This important reservation discloses Lotze's most characteristic methodological conviction. The search for truth must be guided not only by respect for a certain type of empirical fact, but also by the inexorable intellectual and moral demand that no violence be done to other aspects of living experience. For any hypothesis or theory, which out of exclusive concern for 'fact' arrives at conclusions destructive of deep-seated æsthetic and religious convictions, violates the demand for consistency and unity. But on the other hand, a single-minded insistence on unity at the expense of the facts is equally undesirable. An acceptable method must meet both requirements simultaneously; we must derive what unity we can from the facts, while interpreting each fact in the light of the achieved unity of experience and knowledge.

This scrupulous concern for the intuitions of æsthetic, moral and religious experience is readily understood when one calls to mind that for Lotze and his contemporaries in the mid-nineteenth century 'science' was practically synonymous with 'mechanism,' which in ontology spelled a naïve materialism of the sort sponsored in that day by Büchner, Haeckel and Vogt.

[12] Medicinische psychologie, Leipzig: Weidmann'sche Buchhandlung, 1852, p. 8; *cf.*, pp. 32, 65.

This view entailed the rejection of any but physical causes and physical forces from its data, admitting to 'reality' only that which was constructed of physical particles. Lotze was the determined foe of any such narrow formulation, whether it reared its head in the guise of materialism *or* idealism. His training in physiology and medicine predisposed him to champion natural science and mechanistic explanation within proper limits. But mechanism in the above sense could never be the final or ultimate ground of organic phenomena for Lotze; it served simply as the universal and essential conveyor and facilitator of events initiated by some more deep-seated power.

Applied to psychology this interpretation of mechanism compelled the rejection of the cerebralistic hypothesis or what James termed 'conscious-automaton theory,' in its radical materialistic form. To quote Lotze in the succinct and elegant original: "Der gegenwärtige Zustand unserer Kentnisse über den feineren Bau des Nervensystems erlaubt uns keineswegs, seine Beschreibung als eine irgend förderliche Grundlage psychologischer Untersuchungen vorauszusenden." [13] But Lotze, exercising his characteristic prudence, does not fly into the arms of a naïve speculative spiritualism. While he assures us that "only just so much and just such things exist in the world, as are necessary to the meaning of the value-laden Idea, which is the world's essence," he warns that this basic certainty is methodologically barren; for we have no absolute knowledge of either the Idea or of the nature and form of the individual soul in virtue of which the Idea becomes operative and efficient. "It is impossible to determine *a priori*, what general laws the activity of the soul will follow; they must be discovered by reference back to experience. . . ." [14] Therefore, all that *practical* psychology need postulate is a dualism of soul and body and a mechanism for their interaction; and the task of practical psychology is the explication of the interacting mechanism and the formulation of the laws governing its activity. The ultimate instigating power, or soul, must be assumed to be present, but

[13] *Op. cit.*, p. 169.
[14] *Op. cit.*, p. 160.

its nature is a mystery that practical psychology need not fathom. In order ". . . to predict the appearance of a certain event out of given circumstances it is seldom necessary to have a knowledge of the real dynamic powers, . . . ; enough if one knows the formula for the laws actually governing their combined appearance." [15]

But Lotze was too thoroughly steeped in the metaphysical tradition to rest content with psychology at this descriptive and correlating level. The problem of the genesis of mental energy, of that real power manifested in the different departments of mental life, bobs up again and again, and leads Lotze to assert the existence of souls as a necessary inference from the facts of mental life. The notion of the soul serves as a unifying conception; it represents the whole of energy manifested by the separate mental acts, and provides a meaning terminus for psychic and psycho-physic phenomena. The determination of the nature of the soul is the business of *rational* psychology, 'the science of all being and process.' The seeming ambiguousness and the vacillating character of Lotze's ideas results from deliberately mixing the speculations of rational psychology with the empirical facts of practical psychology.

The careful reader of the psychological writings of William James will recognize that, with the exception of a somewhat more stern attempt to keep practical psychology 'pure,' the position just outlined is practically identical with his. In the nineties James disputed the subject of the aims and methods of psychology with Lotze's most faithful American disciple, G. T. Ladd; this dispute provided James with occasions for frequent and emphatic statements of his views.[16] In spite of repeated assertions on his part that seem to contradict the Lotzean position, he never succeeded in cutting loose altogether from the *Problemlage* he had absorbed from him.

[15] *Ibid.*, p. 5. This work is subtitled 'Physiologie der Seele,' indicating that it was to be a study of the soul and its functions as seen through its (mechanically conditioned) effects.

[16] Principles, 'Preface'; Briefer course, 'Introduction'; and the following articles: Review of Ladd's 'Physiological psychology,' *Nation*, 1887, **44**, 473; A plea for psychology as a 'natural science,' *Phil. Rev.*, 1892, **1**, 146–153; Review of Ladd's 'Psychology, descriptive and explanatory,' PSYCHOL. REV., 1894, **1**, 286–293.

There are many passages in which James urges a pure naturalistic psychology in wholly unambiguous terms. In the introductory chapter of the 'Briefer course' he demands that psychology proceed on the working hypothesis "that mental action may be uniformly and absolutely a function of brain-action, varying as the latter varies, and being to the brain-action as effect to cause." In the Preface to the 'Principles' he formulates his own methodological premise as follows: "This book . . . contends that psychology when she has ascertained the empirical correlation of the various sorts of thought or feeling with definite conditions of the brain, can go no further . . . that is, as a natural science." And he advises: "We need a fair and square and explicit *abandonment* of such questions as that of the soul, the transcendental ego, . . . etc., by the practical man; and a fair and square determination on the part of philosophers to keep such questions out of psychology." [17]

But James, like Lotze, was haunted by metaphysics, and whereas he was much more vocal in banishing it from psychology, he succeeded but little better than did Lotze in actually doing so. That the rejection of metaphysics may in itself imply a metaphysics of a certain sort, James realized only too well. He was, therefore, at some pains to dispel the materialistic implications of his professed method and doctrine. "Let not this view be called materialistic," he warns.[18] And lest the reader be in doubt as to his metaphysical position he confides: "My only reason for beating the bushes so . . . has been that by this procedure I might perhaps force some of these materialistic minds to feel more strongly the respectability of the spiritualistic position. . . . I confess, therefore, that to posit a soul influenced in some mysterious way by brain-states and responding to them by conscious affections of its own, seems to me the line of least logical resistance, so far as we have yet attained." [19]

It would be difficult to frame a better statement of the Lotzean position than this last, and there can be no reasonable

[17] *Phil. Rev.*, 1892, 1, 149–150.

[18] Principles, II, 453. This remark is repeated in 'Briefer course.'

[19] *Ibid.*, I, 181.

doubt that Lotze's critical examination of mechanism and the primacy he gave to the soul as a teleological unifying principle made a profound impression on James. It made James aware of the provisional and hypothetical nature of the foundations of scientific psychology, and it led him to disavow any claims of finality for psychology as a natural science.

The chief general differences between the views of James and Lotze on this head are not, therefore, the result of an acceptance or rejection of metaphysics; nor are they the result of embracing spiritualism or conteming it. Instead, the differences arise from the fact that Lotze's metaphysics is monistic whereas that of James is pluralistic. James is content to fall back on a multitude of mental states, whereas Lotze is compelled to seek a principle of unity among them. But even this basic difference is not absolute. For James describes the mental state level as a 'provisional halting place,' and adds that "things must some day be more thoroughly thought out." [20] What direction this thinking-out would take may be surmised from the following statement: "That theory will be most generally believed which, besides offering us objects able to account satisfactorily for our sensible experience, also offers those which are most interesting, those which appeal most urgently to our æsthetic, emotional, and active needs." [21] Nothing could be more eloquent testimony to Lotze's influence on James than the general position outlined in this statement.

III

One of the special problems treated at length in James' 'Principles,' and in connection with which Lotze's doctrine of 'local signs' is exhaustively examined, is the problem of localization and space perception. Lotze was by no means the dominant influence in shaping James' position respecting this problem.[22] In fact, concerning fundamentals they are completely at loggerheads. James labeled his own position

[20] *Op. cit.*, I, 182.

[21] *Ibid.*, II, 312.

[22] On p. 282, of 'Principles,' II, James gives a list of authors from whose account of space he 'derived most aid and comfort.' Lotze's name is not among them.

'nativistic' or 'sensationalistic.' Following Stumpf he identified Lotze's view with the 'psychical stimulus' or Kantian theory, which entails the production of space out of non-spatial elements.[23] James was unable to square this speculative dogma with the demands of a 'practical' and scientific psychology. But this disagreement on fundamentals by no means precludes accord on many significant details.

James consistently maintains that there is an original, primitive, sensorial experience of simple vastness or extensity, which serves as the datum for all further discriminations and constructions of space. A vague spatial quale may be immanent in a single isolated sensation, but the feeling of place or locality requires something more. "Only when a second point is felt to arise can the first one acquire a determination of up, down, right or left, and these determinations are all relative to that second point." [24] When such a second point does arise, it must be felt as qualitatively distinct from all other points, if it is to be experienced at all. To account for this qualitative heterogeneity and the derivation of locality therefrom, James has recourse to the Lotzean doctrine of *Localzeichen*.[25]

It may not be amiss to call to mind the main outlines of this celebrated doctrine. Lotze maintained that several types of sensation, particularly visual, auditory and tactual, are accompanied by a special and distinctive quality known as the local sign, which serves as the place-index of the experience. He describes this sign as a physical nerve process, "regularly associated with those variable nerve processes which form the basis of the qualitative content of the changing

[23] This is hardly the place to enter into controversy over the correctness of attributing the psychical stimulus view to Kant. But it is difficult to understand how this interpretation can prevail in the face of Hermann Cohen's withering criticism in 'Kant's Theorie der Erfahrung,' 2nd ed., Berlin: Dümmler, 1885.

[24] Principles, II, 154.

[25] James' account of this doctrine in his 'Principles' (II, 157 *ff.*), is very apparently based on Lotze's 'Mittheilung' to Karl Stumpf, published as an 'Anhang' to the latter's, 'Über den psychologischen Ursprung der Raumvorstellung' (Leipzig: S. Hirzel, 1873), on Stumpf's criticism of the same, and on Chapter IV of the third book of Lotze's 'Metaphysik' (1879). Beyond reasonable doubt, James saw Lotze's space-perception doctrine through Stumpf's eyes, and it was he who awakened him to a recognition of both the merits and demerits of this account.

sensations." [26] But an isolated local sign is not an absolute index of locality; it is a potential one which can be realized if and when the proper conditions obtain. First among these conditions is a general mechanism that makes possible a comparison of points. In the case of visual experience, eye movement serves this purpose.

Up to this point James sees eye to eye with Lotze; but in the further development of the doctrine a subtle but ever-widening divergence set in. This becomes apparent when the two accounts of the visual space perception mechanisms are compared. James agrees with Lotze (and Stumpf) that a light stimulus affecting any point *P* on the periphery of the retina, will cause a specific reflex eye-movement *p*, tending to bring the stimulus to the fovea. For the practiced eye, actual movement is not necessary; memory and association with earlier eye-movements are effectively substituted, thus enabling the eye to have a *field* of simultaneously localized qualities. It is on the question of the exact relation of these movements and tendencies to movement to the local sign, that the accounts of James and Lotze split. Lotze holds that the movements and tendencies referred to awaken certain *Spannungsgefühle* (in Stumpf's apt phrase) or feelings of tension, and these then are the ground and source of the spatial quale proper, and of the construction of a system of space coördinates. Hence Lotze generalizes: "Everywhere the extensive must be converted into the intensive and out of the latter the soul must reconstruct a new inner space-world." [27]

If James could possibly have missed the implications of this view, his study of Stumpf's remorseless and hard-bitten criticism of it most emphatically exposed its general bearings. The notion that experienced space is a reconstruction of real space through the medium of non-spatial qualitative tokens, constitutes a beautiful case of what James called (in another connection) the 'psychologist's fallacy'; that is to say, substituting the conditions and products of analysis for the real

[26] Medicinische psychologie, p. 331.

[27] Medicinische psychologie, p. 328; *cf.*, Metaphysik, Leipzig: S. Hirzel, 1879, paragraph 283.

nature and process of the analyzed. Believing that sensations are 'first things in consciousness,' James could not countenance a view that involved the assumption of pre-sensational and pre-conscious mental elements. "My space intuitions occur not in two times but in one. There is not one moment of passive inextensive sensation, succeeded by another of active extensive perception, but the form I see is as immediately felt as the color which fills it out." [28] And, ". . . why sensations themselves might not be the soul's *original* spatial reconstructive acts Lotze fails to explain." [29]

It is obvious that this difference is essentially that of the degree to which the two authors are willing to admit 'soul-causality' into practical psychology. Lotze regards psychology as a sort of phenomenology of the soul as seen through its effects. This causes him to interpret all sensation as the soul's reaction to stimulation. Any given sensation is thus the product of a specific stimulus plus the specific reaction which the soul chose to make to just that stimulus, the whole reaction being conditioned by whatever psychical or psychophysical mechanisms are at hand. Where James was content to quarry, collect, describe and correlate mental states, Lotze tended always to view these as effects of deep-seated causes, together constituting the soul. The overpowering desire for unity, evidenced in all departments of Lotze's thought, here again led him practically to obliterate those boundaries which to James' mind formed the chief bulwarks of a scientific psychology. Here as elsewhere, James eschews Lotze, the monistic idealist, while esteeming Lotze, the empirical psychologist.

IV

Nowhere in James' psychology is Lotze's influence so manifest as in the account of the emotions. The substance of the James-Lange theory, the emphasis on peripheral nerve action, the resonance theory of the relation of body and mind, and the denial of the existence of a separate cerebral center for emotions—all these typically Jamesian standpoints had

[28] Principles, II, 275.

[29] *Ibid.*, 276. Lotze does attempt to explain this (Metaphysik, paragraph 260); perhaps this seemed gratuitous to James.

already been achieved on a more or less speculative basis in Lotze's 'Medicinische psychologie' of 1852. James differs only in presenting his theory more emphatically and in a much more radical form. That the identities in the two accounts are no accident, and that James leaned heavily on Lotze in this connection, is easily established by a perusal of the Lotzean texts in the library of William James.

James contended that the truly *emotional* elements of an emotion consist in the feeling of bodily responses to a given cognitive content. Any such content will "excite bodily changes . . . so indefinitely numerous and subtle that the entire organism may be called a sounding board, which every change of consciousness, however slight, may make reverberate." [30] Awareness of this disturbance constitutes the emotion proper; incoming nerve currents are thus the true physiological basis of the emotion. These, "perceived like the original object, in as many portions of the cortex, combine with it in consciousness and transform it from an object-simply-apprehended into an object-emotionally-felt." [31] No new principles need be invoked, nor need the existence of separate brain centers or a distinct 'mind-stuff' for emotion be postulated.

This parallels Lotze's account of feeling and emotion both on the mental and physiological sides. Thus he states: "Our thoughts and tendencies differ according as we recline or stand; a constrained and cramped bodily position damps our courage; it is difficult to be reverent in a comfortable and slouching attitude, rage subsides with bodily repose; the hand that smooths the wrinkled brow dispels the vexation it expressed. It would be difficult to determine the limits of this influence; doubtless it extends very far. . . . The serene enjoyment of pleasing proportions is not only an abstract pleasure; for in the more animated, freer breathing, the accelerated heart-beat and the gentle tension of our muscles we feel ourselves lifted up and carried away. . . ." [32]

[30] Principles, II, 450; *cf.*, PSYCHOL. REV., 1894, 1, 516–529.

[31] Principles, II, 474.

[32] Medicinische psychologie, p. 518. Titchener quotes this passage at length in 'An historical note on the James-Lange theory,' *Amer. J. Psychol.*, 1914, 25, 429.

With even greater clearness Lotze develops the resonance conception of bodily responses—a corrolary of the above view. Thus, ". . . weak oscillations of the central organs invariably accompany the mental stream of ideas, not as its causes, but as its effects, being in fact a kind of resonance which the activity of the soul elicits secondarily in the material substratum, for the sake of intensifying the vividness of its ideas." [33]

Physiologically, this resonance calls for the same type of mechanism that James envisaged, namely, a central organ more or less mechanically incited, and manifold visceral and muscular responses, which, conveyed back to the central organ, combine with the original cognition to form the complex mental pattern that is the emotion. And just as in James, this scheme does not require a separate cerebral center for emotion, and tends to emphasize the role of peripheral nervous activity.[34]

In the light of James' undoubted knowledge of these Lotzean doctrines, it is astonishing to find him stating in his 1884 article, "What is an emotion?," that these ideas "grew out of fragmentary introspective observations," without so much as citing Lotze either here or in the long chapter on the emotions in the 'Principles.' E. B. Titchener called attention to this singular fact some years ago.[35] The truth of the matter is simply that James taught nothing and wrote nothing that could not be established in living experience; and such corroboration weighed much more heavily with him than any demonstrable agreement with the history of ideas. Therefore he did not feel called upon to systematically specify the sources of his ideas in past reading, or to undertake a laborious search for parallels in the history of psychology.[36]

Lotze repeats this passage almost verbatim in 'Mikrokosmus' (3rd ed., Leipzig: S. Hirzel, 1878), II, pp. 381–382; English translation, II, pp. 37–38. On the back fly-leaf of the James copy (Microcosmus, English translation by Hamilton & Jones, New York: Scribner & Welford, 1885) we find in James' unmistakable hand, 'James-Lange theory [p.] 38,' a reference to the passage cited above. This establishes James' recognition of the fact that Lotze has here anticipated his position.

[33] Medicinische psychologie, p. 474.

[34] *Cf., Ibid.*, § 38; p. 554; p. 257 *ff.*; § 35.

[35] See *op. cit.* in my note 32.

[36] James never aspired to be a scholar in the technical, academic sense. He lacked the patience and the scrupulous concern for minute detail necessary for that.

In spite of the absence of acknowledgment on the part of James, the striking similarity of the two doctrines, plus the herein established fact of James' knowledge of Lotze's exposition, leads irresistibly to the conclusion that Lotze was one of the great formative influences in James' theory of the emotions. James' position is, in fact, simply a restatement of the Lotzean one, but more clear, incisive and radical; where in Lotze's view the organic responses are still for the most part only contributory to the emotion, for James they become actually constitutive of it. Also, James supports his position with evidence from introspection and pathology of which Lotze had been quite innocent.

V

The central and most characteristic doctrine of James' psychology, and of his philosophy as well, is contained in the statement, "consciousness is in its very nature impulsive." [37] This thesis dominates his interpretation of the function of cognition, his conception of emotion, attention, interest, memory and will. It is also the immediate source of his pragmatism and more indirectly of his final radical empiricism. The fact that Lotze was a powerful factor in shaping this central doctrine, is the real measure of the significance of his influence. Lotze's account of the will loomed so large in James' eyes and rang so true, that his Chapter XXVI in the 'Principles' is essentially a lucid restatement of the Lotzean position, with considerable elaboration, a careful statement of the *a priori* grounds and the introspective and pathological evidence in support of the position, the addition of much new physiological detail, and a clear statement of the pedagogical and moral implications of the view. In this connection James makes explicit acknowledgment of his indebtedness in the following words: "In his admirably acute chapter on the Will this author has most explicitly maintained the position that what we call muscular exertion is an afferent and not an efferent feeling. . . . How often the battles of psychology have to be fought over again, each time with heavier armies and bigger trains, though not always with such able gen-

[37] Principles, II, p. 526.

erals!" [38] And in a communication to Renouvier, James leaves no further room for doubt when he states: ". . . en ce qui concerne mon originalité, Lotze a été, autant que je sache, le premier à formuler clairement la relation entre représentation, volition et mouvement effectué. . . . Mes propres idées se sont formées, . . . par la lecture et de votre ouvrage et de celui de Lotze; de sorte que je n'ai sur ce point ni indépendance ni originalité quelconque." [39]

Both James and Lotze defended the afferent explanation of the feeling of effort and denied the existence of the so-called feelings of innervation. Lotze states in the 'Psychologie,' ". . . in muscle-feeling we do not experience force on its way to produce an action, but we feel only the ensuing effects. . . ." [40] And again in the 'Mikrokosmus,' ". . . what we call impulse is not a volition by which we guide the body, but a perception of its passive state and of the movements arising voluntarily within it, ... Impulse, accordingly, is nothing but the apprehension of being impelled." [41]

The identity of this position with that repeatedly and militantly defended by James,[42] especially against Wundt, is too obvious to require extended citation. After an exhaustive examination of the *a priori*, introspective and pathological evidence, James concludes, "On the whole, then, it seems as probable as anything can well be, that these feelings of innervation do not exist." [43]

[38] *Op. cit.*, II, p. 523. This same footnote, with unimportant verbal differences, also appears in James' 1880 essay, 'The feeling of effort,' *Collected essays and reviews*, New York and London: Longmans, Green, 1920, p. 182.

[39] *La Critique Philosophique*, 1888, n.s. 4me annee, 2, 401–404. James' essay, 'What the will effects' (*Scribner's*, 1888, 3, 240–250), had appeared in translation in *La Critique*, 1, 401–420. Essentially a popular and untechnical account of his views, it contained few references of any sort, and none to Lotze. This is not surprising, for James had already acknowledged Lotze's priority in the 1880 article cited *supra*.

[40] P. 311.

[41] English trans. by Hamilton and Jones, 1888, I, p. 255. *Cf.*, passages on pp. 286 and 332, reiterating the same idea. In his copy of the 'Microcosmus' James wrote 'WJ' in the margin next these two pages.

[42] This point is argued not only in the 'Principles' (II, pp. 105, 236, and especially 492–522), but also in 'The feeling of effort' (1880); 'What the will effects' (1888); Review of Waller's 'Sense of effort' (*Phil. Rev.*, 1893, 2, 69–73); and in the impatient 'Professor Wundt and the feelings of innervation' (PSYCHOL. REV., 1894, 1, 70–73).

[43] Principles, II, 516.

Again, both authors agree that the mental state that precedes movement proper and 'initiates' it is composed of anticipations of the possible effects of a specific movement. When an act is consciously 'willed' there is in mind as a minimum an idea made up of memory-images of earlier post-action sensations, defining what the act is to be or to accomplish. This ideomotor action is the 'type process of volition' for both Lotze and James. Both admit, however, that in certain cases an additional conscious element in the shape of consent, fiat or imperative is also involved. They agree that will-effort is essentially effort of attention—an attempt, that is, to keep a certain *Vorstellung* before the mind, plus, in some cases as noted, a deliberate affirmation of this ideational content. Both maintain that the will drama is, to begin with, the exclusively mental business of choosing between the various available ideas, and that the dominant idea, unless blocked by another idea, passes over into the appropriate movements in the physical mechanism at hand. Finally, both Lotze and James assert the essential freedom of the will, and draw similar pedagogical and moral consequences from the doctrine.

The completeness of this point-for-point correspondence will, it is hoped, be made altogether convincing by drawing a brief conspectus from each account. To begin with Lotze: (1) ". . . The train of ideas in general passes spontaneously into external movement, and a great number even of complex actions take place in this involuntary fashion. . . ." [44] (2) ". . . Our arising, walking, talking, all these things never require a special impulse of will, but are initiated simply by the train of our ideas." [45] But, (3) "the notion of acting must include the peculiar element of approval, permission, or intention, which constitutes the will proper." [46] (4) "Here as everywhere, the will can only elicit the inner psychological conditions, which nature has designated as the starting-point of external action; the execution of the movement, however,

[44] Microcosmus, 1885, I, 255.

[45] Medicinische psychologie, p. 294. James quotes from this same paragraph in 'Principles,' II, 523.

[46] Microcosmus, I, p. 257.

nature has delegated to the unconscious power by which those conditions are obliged to realize their effects." [47] And finally, (5) freedom of will consists in "unrestricted choice between the objects thus put within its reach." [48] Such choice is made possible by the fact that, "The universal course of things may at every moment have innumerable beginnings whose origin lies outside of it, but it can have none not necessarily continued within it." [49]

Correspondingly in James: (1) "We may lay it down for certain that every representation of a movement awakens in some degree the actual movement which is its object; . . . ." [50] (2) "An anticipatory image, . . . of the sensorial consequences of a movement, plus, (3) (on certain occasions) the fiat that these consequences shall become actual, is the only psychic state which introspection lets us discern as the forerunner of our voluntary acts." [51] (4) "The inscrutable psycho-physic nexus," by which the action proper is produced "lies outside the sphere of the will." [52] (5) True, "the soul . . . is at the mercy of material sources for all *possibilities;* but amongst these possibilities she *selects*. . . . I shall therefore never hesitate to invoke the efficacy of the conscious comment. . . ." [53] For, ". . . let all our thoughts of movements be of sensational constitution; still in the emphasizing, choosing and espousing of one of them rather than another, in saying 'be thou reality for me,' there is ample scope for our inward initiative to be shown." [54]

Whereas the similarity in the conclusions of these two

[47] Medicinische psychologie, p. 301.

[48] Microcosmus, I, p. 256.

[49] *Ibid.*, p. 261.

[50] Principles, II, 526.

[51] *Ibid.*, 501.

[52] Collected essays, pp. 187–194.

[53] Principles, II, 584.

[54] *Ibid.*, 518. This is the psychological basis of the 'Will to believe' doctrine as well as of his indeterministic interpretation of the freedom of the will. While Lotze is undoubtedly the most significant influence on the psychological side of this doctrine, it was Renouvier who provided the ultimate ethical motivation behind these ideas. On this point James left little room for doubt. *Cf.*, Letters, I, 147–148. Stumpf has remarked on the essential identity of even this moral aspect of the doctrine with the ideas advanced in Lotze's 'Mikrokosmus.' See 'William James,' p. 31.

accounts is striking, the method and spirit in which they are reached is different. James was not content simply to accept these standpoints on the basis of Lotze's peculiarly mixed *a priori*, empirical and dialectical proof. This fact affords a complete explanation of such otherwise baffling statements as the following: "It was in fact through meditating on the phenomenon in my own person that I first became convinced of the truth of the doctrine these pages present, ..." [55] Lotze's ideas served to give aim and direction to James' search for introspective and objective facts concerning the nature of the will, and in the process James became convinced of the essential truth of Lotze's account.

VI

The foregoing presents the most important, but by no means all of the connections in which Lotze's influence on James is noteworthy and important. On a host of minor and scattered issues James defers to his greatly respected predecessor. For the most part these various references, occurring in such special fields as attention, interest, memory, perception, conception, reasoning, the self, etc., all grow out of one basic conviction which James shares with Lotze, namely, that feeling and will are a more fundamental, more pervasive, and more significant power in human existence than are reason and intellection. This is what was meant earlier in stating that both authors assert the priority of the 'practical' reason.

This fact explains a general but striking similarity in the treatment of a number of special issues. Both authors maintain that attention and interest are essentially functions of feeling and will; in their exercise, we 'carve out' of the total available aggregate that which is relevant to our practical needs and our æsthetic and moral requirements. In order that this may be metaphysically justified, both take their stand on the freedom of the will, as already indicated; both distinctly posit a free spiritual force not itself subject to cerebral laws. Conception and reason are similarly disposed. For both Lotze and James subscribe to the instrumental

[55] Principles, II, 525.

view of concepts,[56] and argue that, epistemologically speaking, while the reasoning process does not necessarily follow actual cleavages in an independent reality, yet the end-results, if valid, must lead to an apprehension of the meaning and value of reality. Both also recognize two attitudes toward the object of knowledge, or two 'kinds' of knowledge. Thus, Lotze's '*cognitio rei*' corresponds exactly to James' 'knowledge by acquaintance'; while the former's '*cognitio circa rem*' resembles the latter's 'knowledge about.'

Again, there are many aspects of James' account of the Self that hark back to Lotze. He discredits the idea of soul-substance and credits the German psychologist with having written 'a most beautiful criticism' of this notion.[57] He argues about the unity of the self: "So long as our self, on the whole, makes itself good and practically maintains itself as a closed individual, why, as Lotze says, is not that enough?" [58] And he agrees with Lotze also in asserting that the self is apprehended not so much through cognition simply, as in feeling. Where Lotze speaks of the '*Innigkeit*' that distinguishes self from not-self, James writes of the peculiar 'warmth and intimacy' that characterizes this object.

And finally, James found in Lotze the encouragement to defend two characteristic standpoints, both resting ultimately on the assumption of the priority of feeling over other modes of understanding. The first of these is the emphasis on mind activity and its efficacy in a world otherwise dominated by mechanical causation. Lotze was fond of dwelling upon the *reactive* character of all mental and physical response to stimulation. A response is what it is because the reacting agent had just this specific nature. Obviously, this makes the *person* and the motor-affective make-up an irreducible fact; it is a reactive organism whose responses are *conditioned*, but not *determined* either by the stimuli which impinge upon it or by the mechanisms through which its selected responses are released into the realm of natural law.

[56] It is interesting to note that John Dewey, early in his career worked out his instrumentalist doctrine by analysis and criticism of Lotze. See, Studies in logical theory, Chicago: University of Chicago Press, 1903, Chapters I–IV.

[57] Principles, I, 349, note.

[58] *Ibid.*, 350.

The other point is broader but closely related. Because both James and Lotze believe that persons and their purposes and interests are primary facts, both make a teleological interpretation of reality. All reasons, if there be any, "must lie in the total sense or meaning of the world," [59] says James. This idea forms the very heart and nucleus of Lotze's monistic teleological idealism. But for both this total meaning of the world is not a *fait accompli*, but something to be realized in the course of living and thinking and feeling.

In conclusion, it is important to note that with but one exception Lotze influenced James exclusively on the 'Dionysian' side of mental life, not on the 'Apollinian.' The one exception is James' highly qualified acceptance of the doctrine of local signs. The 'Dionysian' elements of Lotze's psychology rest in the main on scientific introspection, not on the idealistic premises of the speculative *Deutsche Bewegung* of the late eighteenth and early nineteenth centuries. While it is true that Lotze does eventually arrive at a conception of the relation of the 'practical' or axiological to the theoretical phases of experience that is much like Kant's, he differs from him in determining the concrete nature of the *Gemüth* on empirical, not on *a priori*, grounds. The theoretical, rational, cognitive or Apollinian phase of Lotze's doctrine bears much more likeness to his forebears, particularly to Leibnitz and Kant. It is, therefore, entirely natural that James, with a temperamental revulsion against the austerity and bloodlessness of monism, should feel most attracted to the original, realistic and empirical elements of Lotze's thought.

But it was not in James' make-up to accept any concept, idea, or theory on the basis of authority, tradition, or respectability. Everything and every stimulus that he received from Lotze as from anyone else, he first subjected to criteria and tests of his own before incorporating them into his writing and teaching. As Josiah Royce has so admirably put it: "He, too, has been widely and deeply affected by the history of thought. But he has reinterpreted all these historical influences in his own personal way. He has transformed

[59] *Ibid.*, 401.

whatever he has assimilated. He has rediscovered whatever he has received from without; because he never could teach what he himself had not experienced." [60]

[60] William James and other essays, New York: Macmillan, 1911, p. 7.

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AN INTERPRETATION OF INHIBITION OF CONDITIONED REFLEXES AS COMPETITION BETWEEN REACTION SYSTEMS [1]

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I. Introduction

This paper presents new evidence in support of the point of view that the inhibitions of complex behavior, like those of the spinal animal, are one aspect of the process of competition between reaction systems. That is to say, *an activity is inhibited when some other behavior system takes its place.* This point of view is already well-known for it has been espoused by a number of writers. McDougall (**6**) has systematically developed it as support for his drainage theory of inhibition. Dodge's Laws of Relative Fatigue (**2**) and the problems of a number of his experimental papers (**3**, **4**, et al.) are expressions of it. Various reviewers (Washburn, Razran, Guthrie) have called attention to it. It has found expression in psychopathology (W. S. Taylor and others), in the psychology of motivation, and in fields outside of psychology.

The view that "inhibition in the brain is always the negative collateral aspect of a process of excitation" (**6**, p. 250) is reinforced both by evidence in its own right and by the prejudice that the principles which have been established for the reflex functions of the simplified nervous system (such as the spinal preparation) should, so far as possible, be used in the explanation of more complex behavior. The recent collaborative book of the Sherringtonian group on 'Reflex

[1] It would give me great pleasure to be able to acknowledge each instance of indebtedness which has contributed to the views expressed in this paper. My attention was first called to the problem of inhibition by Professor F. H. Pike. My debt is especially great to Professor Raymond Dodge. But during the five-year period of my membership in the Institute of Human Relations—Yale University School of Medicine group I have incurred so many other debts, especially to the younger research workers in the group, that any list of them would be likely to be incomplete.

activity of the spinal cord' (1) shows that all of the inhibitions of the spinal cord are reciprocal inhibitions. Leg extension, for instance, is inhibited by stimulating leg flexion. The scratch reflex system is inhibited by pinching the paw of the opposite hind leg, thereby eliciting an antagonistic reflex system. This Principle of Reciprocal Innervation is applied by them not only to the finest details of reflex coördination, but also to the reciprocal relationships existing between reflex systems. The scratch reflex *system*, involving a complicated picture of reciprocal relationships in the action of the muscles of neck, trunk, fore-limbs, opposite hind limb, and active scratching limb, may be reciprocally inhibited in its entirety by the flexion reflex *system* elicited by pinching the paw of the hind leg on which the animal is standing. The use of the Principle of Reciprocal Innervation as applying to the interaction of reflex systems is of especial importance for this paper.

Opposed to this view is that which has been given wide currency by Pavlov and which has apparently been accepted by many psychologists. The Pavlovian view of inhibition regards it as an *independent* process. It is further described as an irradiating condition showing stages of generalization and concentration. This irradiation is "regarded as the result of the spreading of the inhibitory process from its point of initiation through the entire structure of the hemispheres" (7, p. 61). It is as though there were a 'spreading of the waters of inhibition to extinguish the fires of neural excitation.' The index of this spread is the progressive decrement of certain behavior items.

The evidence to be presented in this paper has led the writer to the extension of the Principle of Reciprocal Innervation to the inhibitory phenomena of complex behavior. This evidence is chiefly from two groups of experiments: the first on habituation to rotation and the second on inhibition of conditioned food-taking reactions of monkeys. In the experiments on habituation to rotation the nystagmus of the eyes disappeared on repetition of stimulation (negative adaptation), and in the experiments on conditioned food-taking reactions of monkeys behavior occurred which could be

classified as 'inhibition of delay,' 'extinctive inhibition,' 'internal inhibition' and 'external inhibition.' If Pavlov's view of inhibition is correct, we should expect inhibition of one act to be accompanied by a spread of this condition to other acts. If, on the other hand, complex behavior follows the same principles as the behavior of the spinal animal, the inhibition of one reaction system would be but one aspect of the shift of activity to another reaction system. In the experiments about to be described the latter was found to be the case.

Pavlov's theories are rejected on the ground that they are not supported by physiological evidence and that the data to which he applied them can be explained in other ways. His results on dogs differ from our results on monkeys (described in section III below) in that the dogs showed what is described as a generalized inhibition while the monkeys clearly did not. Reasons will be given in sections IV and V below for believing that the behavior of both the dogs and the monkeys can be interpreted as instances of competition between reaction systems. We believe that the differences between the results of his experiments and of ours follow from the special conditions of each experiment.

II. Experiments on Habituation to Rotation [2]

Dodge's study of habituation to rotation as manifested in eye-movements (3) led him to interpret that process as one in which two response systems are in rivalry or competition. The originally dominant system, that of vestibular nystagmus, loses out in this competition to the system of still-fixation. The present study has confirmed his interpretation and amplified the facts. By resorting to binocular records, by taking the records at greater speeds of recording paper, by concentrating the study on short arcs of rotation, and by

[2] The experiments described in this section were done while the writer was a Fellow of the National Research Council at the Institute of Human Relations, Yale University, working under the sponsorship of Professor Raymond Dodge (1930–1932). Some details of the findings were reported in 1931, 1932 and 1934 (9, 10, 11). A complete description of the experiments will eventually be prepared for publication. For the purposes of the present paper only the main result will be mentioned.

studying the effects of various antecedent conditions, it was possible to carry the analysis further and to clarify matters which his records did not fully reveal.

Apparatus and method.—Human subjects, placed on a rotating chair,[3] were repeatedly rotated until habituation occurred. Horizontal eye-movements were photographically recorded through closed lids by means of Dodge's mirror recorder. Habituation to a number of types of rotation and under various conditions was studied in different individuals (total of 102 subjects). Similar observations were made of habituation to rotation in monkeys, by placing the immobilized animal on a rotating platform and recording its eye-movements by means of the mirror recorder.[4]

Results.—Most of the observations were made on habituation to repeated rotations through short arcs (15 to 65 degrees). Under these conditions the original response to rotation is a nystagmoid eye-movement during the rotation, with little after-movement. The well-known pattern of a slow eye-movement compensatory to the rotation, interrupted by rapid return movements is found. With very short arcs of rotation (15 degrees) the fast phase may in some cases be small or infrequent.

Now, according to the Pavlovian view, habituation is by inhibition. This reflex should therefore gradually be inhibited until it disappears. We should expect a gradual decrement in the angular velocity of the compensatory eye-movement until finally no movement takes place upon rotation.

Such a gradual extinction of the vestibular reflex does not take place. Instead of being simply inhibited, the reflex eye-movement is *displaced.* Another eye-movement system gains dominance and the vestibular system is thereby reciprocally inhibited. The objective and functional characteristics of this intruding eye-movement system have been studied in some detail, and attempts have been made to discover its peripheral and central controls. In the early stages of

[3] A picture of the apparatus used was published by G. L. Freeman, Introduction to physiological psychology, 1934, p. 357.

[4] The experiments on monkeys were done in collaboration with Dr. Margaret A Kennard. They have not yet been prepared for publication.

habituation the new eye-movement system achieves only an incomplete and partial dominance over the vestibular system. It breaks into the vestibular reflex only momentarily at first, and not until later does it achieve complete dominance. The active nature of the competing system is shown most clearly in records where the eyes, after habituation, actually move in the opposite direction from that required by the stimulation. Instead of making a compensatory response at onset of rotation, an opposite response may be made so that the eyes move toward the direction of rotation. The normal unhabituated eye-movement response to slow oscillation of the rotating chair is a compensatory oscillation of the eyes in a direction opposite to the rotation interrupted by some saccadic movements. The form of the slow phase of this oscillation may mirror the form of the rotation with remarkable fidelity (*Science*, **74**, p. 608). At a certain stage of habituation this reflex is replaced by an ocular oscillation which is the inverse of the original, so that the record again shows an oscillatory pattern of eye-movement, but in the opposed direction.

The normal unhabituated response to a rapid short rotation to the right (65 degrees) is a compensatory drift to the left interrupted by several saccadic movements to the right. At a certain stage of habituation this reflex is replaced by an irregular or tremulous drift to the right at onset of rotation. The compensatory movement has been replaced by an antagonistic eye-movement system which moves the eyes toward the direction of rotation so that they move ahead rather than lag behind. This new type of eye-movement differs from the reflex in that it does not have the nystagmoid pattern (*i.e.*, it does not contain fast phases), it is less regular and smooth, it is often out of phase with the rotation or may bear no relation to it, and in some instances it perseverates after the cessation of rotation. (This perseveration is most easily identified after oscillation, when the eyes may continue oscillatory horizontal movements for several seconds.)

Conclusion.—These experiments show that the inhibition of vestibular nystagmus during habituation is similar to the

inhibitions of the spinal cord. The vestibular reflex is reciprocally inhibited when a rival eye-movement system gains dominance in much the same manner that the scratch reflex is reciprocally inhibited when the contralateral flexion reflex gains dominance.

III. Experiments on Inhibition of Food-Taking Responses of Monkeys [5]

In experiments on the auditory capacity of monkeys, undertaken in collaboration with Professor J. G. Dusser de Barenne, observations have been made on the nature of inhibition in a quite different situation from that of the negative adaptation of a reflex. The object of these experiments was the measurement of the animals' auditory acuity, but incidentally they afforded an opportunity to make the observations here recorded.

Apparatus and methods.—The details of the apparatus and method appear in a previous paper (**12**). In brief, the method is as follows: Each animal is trained to respond to the onset of a tone by opening a drawer to get a piece of food. The animal is confined in a cage with a barred aperture (grille) through which it can reach out to grasp the handle of the food-drawer. The grille can be closed by a roller curtain attached just beneath the grille, so that raising of the curtain closes the grille. The experiments are done in a small, sound-shielded, acoustically treated room. The animal is in a cage with sides of wire mesh. The front of the cage is enclosed except for the grille through which the animal works. Above the food-drawer is a loud speaker mounted on a baffle. This baffle effectively divides the room into two compartments, one for the experimenter and one for the animal. The experimenter keeps the animal under observation by means of appropriately placed mirrors. The situation confronting the

[5] The observations described in this section were made during experiments on the auditory acuity of monkeys and on the effects of cortical lesions on auditory acuity. Some of the results here reproduced have already been alluded to in a previous publication (**12**). In that publication appears an extensive discussion of behavior mechanisms closely related to those here examined (**12**, pp. 36–45). The numbers assigned to the animals whose data appear in this paper are the same as those used in the preceding paper.

animal is this: At approximately one minute intervals a ready signal is given and five seconds later the curtain is opened, giving the animal access to the drawer. It eventually learns to come to the grille when the curtain opens, waits there in a listening attitude until the tone occurs, then promptly opens the drawer and takes out a piece of food. The tone occurs either 8 seconds or 16 seconds after the curtain opens, the delay interval being changed in a unpredictable order. If the animal responds before the onset of tone, the drawer is empty. It therefore learns to inhibit response during the delay period.

Important variations of this procedure are described in (**12**, pp. 13, 17, 18, and 25). These include the use of longer delay periods with some animals.

Eleven monkeys have been used, the species being baboons, mangabeys, spiders and rhesus. In the case of seven of them the training extended over long periods of time, being as long as 4914 trials, 235 experimental sessions, and 21 months of work. The facts here reported were recorded in the form of protocols made during the training and subsequent investigation of the auditory acuity of the animals. The data are largely non-quantitative, their quantitative aspects having been previously reported (**12**). Details of the history and of the peculiarities of the animals are also reported there.

Results.—If the Pavlovian laws of behavior are true of monkeys in this situation, we should look for symptoms of an irradiating and generalizing inhibition affecting other response systems. The results of these experiments show, to the contrary, that the animals achieve inhibition of response to the drawer by filling the delay period with other substitute responses *natural to the situation.* The inhibition of response to the drawer may therefore be looked upon as being reciprocal to these other response systems. The behavior of the monkeys will be contrasted with that of Pavlov's dogs for several of the varieties of inhibition of conditioned reflexes.

A. The Inhibition of Delay.—In the Pavlov experiment the inhibition of delay occurs when a conditioned stimulus is given and then the dog waits until he hears food drop into the

food pan. In our situation the curtain opens and the monkey waits until he hears a tone which is the signal to take food. The sight of the food drawer then acts as the conditioned stimulus and the tone followed by food acts as the reinforcement after a delay. Pavlov's dogs are described as follows: "The animal remains completely indifferent and quiet in the first period of action of the conditioned stimulus; or even (as is not seldom the case) immediately upon the beginning of that stimulus, he drops into a drowsy and sometimes abruptly into a sleeping state, with relaxation of the musculature and snoring. This, on entering the second period of the conditioned stimulus, is replaced (sometimes with a start) by a clearly suitable conditioned motor reaction" (**8**, p. 95).

No such condition has been observed in the case of monkeys. They first achieved the inhibition of delay by staying away from the grille and busying themselves with other activities, or by engaging in some substitute activity at the grille. These other activities are interpreted as operating as active competing tendencies which achieve dominance over the response to the food-drawer. The substitute activities which took the place of response to the drawer were, as one would expect, those activities which are natural to the situation. If we put a monkey into the experimental cage, he will manifest certain types of behavior peculiar to himself and to the species to which he belongs. In the case of rhesus monkeys fear responses and grooming responses have been most prominent in their cage behavior. The spiders occupied themselves mostly with scratching themselves, with rough manipulations of parts of the cage, and with play involving swinging from the cage sides or top. The cage behavior of one of our mangabeys (No. 7) was predominantly locomotor and manipulative, the manipulative behavior having very great variety. The cage behavior of our baboons included a greater repertory of activities: manipulative acts, rough play—often destructive to the experimental cage—, vocalizations, galloping in place, and attacking the wooden frame of the cage with their teeth were most prominent. When inhibition of response to the food-

drawer occurred, it was as one aspect of a shift of behavior to these other response systems natural to the experimental situation.

Some of the substitute activities noted in the protocols of the early part of the training are the following: A baboon (Monkey No. 1) achieved her first delays by climbing the sides of the cage, by general activity and noisiness in front of the grille, by biting at the cage sides or pulling at the wire, by vocalizations, and occasionally by hanging upside-down on the bars of the grille. A rhesus (No. 2) and a spider (No. 3) inhibited response to the drawer by sitting in the rear of the cage and scratching or grooming, vocalizing or playing with parts of the cage. The early inhibitions of another spider (No. 4) were uniformly accompanied by continuance of the animal's natural cage behavior. Instead of hurrying to the grille when the curtain opened, he remained hanging from the side of the cage scratching himself or poking at objects outside the cage. The nature of the inhibitions was especially clear in the instance of a mangabey (No. 7). Although she might be quiet before the grille was opened, immediately on the opening of the curtain she began circling about the cage (perhaps hesitating at the grille on each circuit) and thereby refrained from reaction to the drawer until the onset of tone. At other times, instead of rushing to open the drawer, she attacked the wooden frame of the cage with her teeth or pulled vigorously at the curtain cords. Two baboons (Nos. 8 and 11) gave clear-cut instances of inhibition by substitution. Their early delay periods were achieved by acrobatics on the cage sides or top, or by galloping in place and grunting. These response systems were definitely elicited by the opening of the grille.

As the training went on, the nature of the substituted responses underwent a change in that the animals more and more tended to fill the delay period with responses which could be made in the vicinity of the grille. General activity and noisiness just at the grille, vocalizations, scratching themselves, biting at the bars of the grille, manipulation of the curtain cords, sides of the food-box and front of the baffle,

placing of the foot or tail (in the case of the spiders) on the drawer handle, and other such responses served as inhibitors of response to the drawer.[6]

In the case of those animals whose training continued to the point where accurate limen determinations of response to tone were possible, the nature of the behavior in the delay period underwent a still further change, but this change was still within the pattern of inhibition by substitution. In this period of the training those substitute responses which were of such a nature as to interfere with hearing tended to drop out. More and more the activity of delay became a closer approximation of the response of drawer opening. In this period our monkeys fit well into the descriptions given by Woodrow (**13, 14**) of the behavior of his monkeys in a somewhat similar situation. All of our trained monkeys frequently, and some of them almost invariably, filled the delay period with responses approximating the drawer opening response. These were such acts as batting at the handle, reaching out and touching it, sliding the hand over it, giving it a slight pull insufficient to open it, or waving the hand in the air in the region of the handle. Even when the animal did not reach

[6] A particularly interesting and illuminating behavior sequence developed in the case of a baboon (No. 11). When her training was resumed in the fall of 1934 (at that time both temporal lobes had been removed) her reaction to the drawer was characterized by a very high frequency of premature responses, *i.e.*, responses before tone. This continued for 29 experimental sessions. During the 29th session 17 out of 20 trials were occupied by premature response. At the 30th session a new behavior sequence developed. During the delay period the animal began to grasp the curtain with both hands and vigorously pull and shake it. The frequency of premature response dropped from 85 per cent to 45 per cent. During the 31st session it dropped to 5 per cent, *i.e.*, there was but one premature response in 20 trials. This premature response occurred on the only trial in which the animal failed to occupy herself with the curtain. The animal thereafter continued this method of working, and is still doing it at the time this paper is being written, three months later. Now, however, the behavior has undergone a most interesting change. Instead of merely shaking the curtain the animal grasps its edge, unrolls about 18 inches of curtain and holds the curtain so as to hide the food-drawer from her sight. Sometimes the animal will hold the curtain with one hand, and when it chances to drop below her eye line, she reaches over it with the other hand to open the drawer. Her behavior is tending toward a slightly modified method of hiding the drawer. Sometimes the animal unrolls a part of the curtain and uses it to cover the drawer by pushing it forward so as to cover the drawer handle. This, in a rather extreme form to be sure, is a true example of the 'inhibition of delay.'

for the drawer its arm was usually in motion and the attitude of the whole animal was tense and alert. The head was constantly in slight motion to sample the sound field. This listening attitude itself is interpreted as an active response system in rivalry with the drawer opening response system.

To summarize this material, the inhibition of delay first occurred when the animals made other responses during the delay period, and when well-developed was characterized by activity which was in rivalry with the response to the drawer. In this situation the 'inhibition of delay' might better be called the 'activity of delay.'

B. Extinctive Inhibition.—On the matter of the inhibition of extinction two questions will be raised. (1) Is it a general rule that extinctive inhibition shows an irradiating generalization? Our experimental answer to this question is, "It does not." (2) Does the response in question disappear (*i.e.*, become extinguished or inhibited) because of (*a*) some factor tending to depress it directly, or (*b*) reinforcement of competing response systems? This question is answered by, "We do not know." In the following discussion we will consider first the data on the first question.

Pavlov states, "That extinction is actually inhibition, is proved as well by its successive inhibitive effect on other positive conditioned responses as by its transition to drowsy and sleeping states, which is without doubt inhibition" (**8**, p. 99).

Again the behavior of monkeys in this experiment does not correspond with that of Pavlov's dogs. When a monkey makes a premature response before tone, it is unrewarded, and this usually has an 'extinguishing' effect on such responses. There is, however, no evidence of any inhibitory effect on *other* responses. On the contrary, there is more likely to be increased activity. This reinforcement of other activities may then operate to favor them in the competition with response to the drawer. A premature response very frequently gives rise to vocalizations such as crying, chattering or barking. In other instances there is definite rage-behavior such as biting at the drawer, pulling vigorously and repeatedly

at the handle, or screaming. Five of the animals often pushed the drawer back into place after a premature response. The general activity after premature response was relatively high, and characterized by racing about the cage, pulling at the curtain ropes, punching at the curtain, and general excitement. These directly exciting effects of a premature response were in turn displaced when the animals again resumed those normal cage activities characteristic of themselves.

The excited or rage behavior following a premature response sometimes resulted in an upset of the balance of competing tendencies to drawer opening and refraining from drawer opening so that the consequence of a premature response operated to reinforce that mode of responding instead of having an extinguishing effect. To support this statement it is possible to point to certain quantitative aspects of the data, in instances when premature responses were made on two or more consecutive trials. Within such groups of premature responses there tended to be an initial period of decrease of latency followed by a period of increase in latency until the animal delayed long enough for a tone to be presented. This reinforcing effect of premature responses was greater during the period of training than after the animal was thoroughly accustomed to the procedure. Tabulation of the data of four monkeys (Nos. 1, 3, 4 and 7) and curves of the average latency of successive premature responses show that a reinforcing effect of premature response was prominent in the case of three of them (Nos. 1, 3 and 7), but did not appear in the averaged data of the fourth (No. 4).

It was only with very timid and fearful animals (No. 2 and 10) that premature response resulted in a definite decrease in the amount of overt behavior, and even in their cases it was by no means a uniform phenomenon. After several premature responses these animals often retired to a corner of the cage to occupy themselves with grooming. We have never seen any indication of relaxation or sleep.

In summary of this part of our data bearing on the problem of the inhibition of extinction, it can be said that they indicate a *redirection* of activity into other response systems rather than a generalized *reduction* of activity.

This immediately leads to the second question we asked about extinctive inhibition, namely, whether the loss of response is due to factors tending to depress it directly or to reinforcement of competing reaction systems. The various possible circumstances which may precede a shift of dominance from one behavior system to another are discussed in the last section of this paper, but some pertinent aspects of the data may be examined now.

Rage behavior and excited behavior, having their origin in the absence of reward, are, or course, not necessarily the reactions which lead to a successful delay on the succeeding trial. In addition to the elicitation of these behavior systems, the absence of reward also has the effect of favoring those substitute responses which are normally in competition with the drawer-opening response. But whether the competing responses are favored because the premature response directly *reinforces* them, or whether the premature response sets up some barrier against its own repetition and thereby leaves other responses *relatively* more excitable and hence able to gain dominance, is not clear. Still another possibility is indicated by the following observations: In the case of three of the animals a premature response occasionally resulted in the appearance of an old fear reaction to the situation, and they leapt away from the empty drawer as though from punishment. This was the case with Monkey No. 1 who had been trained with the aid of an electric shock administered from the drawer handle and the fear reaction manifested itself clearly even 12 months after the use of shock was discontinued. In the cases of Monkeys No. 2 and No. 10 the fear response was originally to the situation itself. A reason cannot easily be assigned for its elicitation by an empty drawer. The fear reaction of each of these animals had been, or still remained, a prominent feature of their reaction to the whole experimental situation. Its prompt elicitation by an empty drawer therefore clearly raises the question as to whether there is actual reinforcement of a response system competing with the drawer opening response system, or whether the fear response appears simply because

the competition of the food-taking response system is removed. The situation appears to be that the animal approaches the food-drawer with two conflicting response systems: approach or food-taking and flight or fear. The food-taking system maintains its dominance to the point of opening the drawer, but as soon as the empty drawer terminates that mode of response, fear promptly becomes the dominant mode of response.

C. Internal Inhibition.—A third instance of inhibition in Pavlov's experiments occurs with the simple repetition of reinforced stimulations. An animal which gives active salivary responses at the beginning of an experiment may give none at the end, in spite of constant reinforcement. Pavlov ascribes this to the development of inhibition. Monkeys occasionally do the same thing; toward the end of a day's trials they may make no response whatever to the food-drawer. In our experience this is not accompanied by a generalized inhibition, but occurs when the animal's play reactions begin to compete with the food-situation for dominance. When the animals are first put into the cage they work at the problem of getting food. After working for a time, they begin to spend more of their time at trying to pull the cage apart, at picking at the acoustical padding on the walls, or at attempts to open the cage door. An increase in general activity is the common accompaniment of this behavior, which in the case of Monkey No. 11 was sometimes so great as to make it necessary to remove her from the cage in order to preserve its integrity. Only in the case of the timid animals (Nos. 2 and 10) was there a decrease of activity, and that decrease was only in the locomotor aspects of their behavior, and not in the manipulative aspects.

D. External Inhibition.—This phenomenon occurs also in the case of monkeys, but here the mechanism is clearly that of interfering response systems. Accidental noises by the experimenter sometimes elicit chattering and attempts to look at the experimenter, thus interfering with (*i.e.*, inhibiting) the food-taking response. All food-taking responses of Monkey No. 1 could be inhibited if the experimenter wore gloves;

the animal was then responding to the experimenter rather than to the food situation. The presence of another person in the room had a similar effect, for the response was then to this person. Thus, an attempt to demonstrate the experiment to one of the Russian reflexologists failed because the animal (No. 4) would not respond. He called this 'inhibition'; we prefer to call it 'interference.'

E. Scattered Sleep.—According to Pavlov "internal inhibition during the alert state is nothing but scattered sleep." He describes transitional stages between concentrated and generalized inhibition where only the motor analyser of the dog is affected. In this condition the dog salivates when the stimulus is given, but the motor responses are lacking and the dog does not take the food. If such an incipient state of generalization of inhibition were present in our monkeys, one would expect that the arm with which they reach for the drawer would be rendered inert by inhibition. No such condition has been observed. Rather, it is the arm which tends to be the most active member during the period of delay.

Conclusion.—These experiments showed that all of the inhibitions found in this food-taking situation can be interpreted as instances of competition between reaction systems and as being analogous to the reciprocal inhibitions of the spinal cord. Inhibition of response to the drawer occurred when other response systems became dominant.

It must be admitted that an explanation of the behavior of these monkeys could, without great difficulty, be made in terms of the Pavlovian theories by means of the principle of *induction.* The description in these terms might, indeed, appear equally as plausible as the description here advanced in terms of competition of reaction systems. However, the supports have for some time past been cut out from under the notion of induction by evidence leading to its abandonment as an explanation of certain phenomena of the reflex activity of the spinal cord. Pavlov borrowed the concept of induction from Sherrington's 'Integrative action of the nervous system,' where it was used in the explanation of such phenomena as rebound. These phenomena are now believed

to be due to delay path action or to be the result of stimulation of mixed reflexes where one reflex is concealed by another dominant reflex. Hence, rather than as an added principle of reflex action, induction is now regarded as only a special case of the operation of the Principle of Reciprocal Innervation. With the evaporation of the basic evidence for induction, it would seem wise to relinquish its use as an explanation of complex behavior.

IV. Pavlovian 'Sleep'—Experiments on Immobilization of Animals

Pavlov has unequivocally identified inhibition with sleep. General relaxation, death-feigning in animals, etc., may also be explained as being instances of generalization of inhibition. It is this type of behavior which apparently offers the greatest difficulty of interpretation in terms of the competition of reaction systems. The argument is immediately brought forward that sleep, relaxation, etc., are not active: that their essential characteristic is passivity. The facts, however, indicate that sleep also has its active side. As McDougall (**6**) has pointed out, the assumption of the sleep posture (a decidedly active posture in the case of some animals) and the contraction of the musculature which closes the eye-lids are two of the active components of sleep.

The passage of inhibition into sleep in Pavlov's experiments is not duplicated in our experiments on food-taking reactions of monkeys. But both situations can be described as instances of competition between reaction systems if we bear in mind that the reaction system of sleep is one of the modes of reaction of which the dog is capable. In Pavlov's situation certain factors favor sleep, while in our situation there are factors favoring play, manipulative acts, vocalization, rage behavior, fear, etc. When, after the procedure of extinction (or some other procedure), the experimental stimuli lose their potency, one may expect the animals to shift to other behavior systems natural to the situation. In our situation the natural reactions are play, manipulation, sometimes fear, and others described above. It is possible that in Pavlov's situation the natural reaction is sleep. The gener-

alized inhibition or sleep which he describes is therefore tentatively regarded by the writer as being a function of a special situation favoring the reaction systems allied with sleep. The special factors which might be operative are: (1) restraint of the animal, (2) absence of varied stimulation with its possibility of varied reaction, and (3) repetitive or monotonous stimulation. Some of the conditions of sleep in dogs have previously been pointed out by McDougall (**6**). They are matters which merit further experimentation. We have obtained some evidence on one of these factors, namely, immobilization of the animal.

In experiments on habituation to rotation in monkeys described earlier in this paper, and especially in experiments on visually elicited eye-movements of monkeys and dogs done in collaboration with Dr. D. G. Marquis (**5**), an opportunity was afforded for observing the consequences of immobilization of the animals. For purposes of recording eye-movements the animal's body was placed in a box restricting its movements and its head was immobilized. The monkeys' heads were immobilized by pressing the teeth into dental impression compound and binding the heads with gauze bandage. The dogs' heads were immobilized by rubber padded braces against various points on the head and snout.[7] A most striking consequence of this procedure is seen in the case of the rhesus monkey (*Macaca mulatta*). The animal becomes still almost immediately it is confined, and within a short time goes into a state having some resemblances to sleep. The spiders (*Ateles ater*) and green monkeys (*Cercopithecus aethiops sabaeus*) show the same behavior, but to a somewhat lesser extent. Dogs show a similar state after immobilization, but apparently are less subject to it than the rhesus monkey, for in our experiments it takes place less often and less promptly, and is more easily broken into by stimulation.[8]

[7] A picture of the apparatus used appears in a paper by Hilgard and Marquis, *J. Comp. Psychol.*, 1935, **19**, p. 31 and in Murchison's 'Handbook of general experimental psychology,' 1934, p. 389.

[8] This condition is apparently also present in cats. Dr. T. C. Ruch tells me that he has found it very useful during experiments on cats and monkeys with low spinal transection. He simply thrust the fore-part of the animal into a golf-stocking (with provisions for breathing) and could then tranquilly conduct experiments without interference from struggling.

The chief external signs of the condition in the rhesus monkey are absence of struggling or movement of the limbs, uniform respiration, partial or complete closure of the eye-lids, elevation and wandering of the eyes and absence of fixational eye-movements. During this condition response to stimulation is difficult to elicit. When the animal does respond to a stimulus, the response is associated with the temporary abolition of the sleep-like condition. Vestibular and visually induced nystagmus are difficult to elicit and of brief duration. The animal will endure a variety of insults without apparent reaction, insults which, in the normally alert animal, would call forth vigorous defence reactions. By increasing the intensity of stimulation, response can usually be elicited. However, the intensity of stimulation is not the only factor determining its effectiveness. The repetition of even very intense noises, etc., soon fails to elicit any perceptible response. But, under these conditions some new stimulus, even though very faint—such as a slight scratching sound, whistling, the sound of footsteps from the floor above, the slight noises of the camera motor, motion in the visual field, darkening of the room, increase in the illumination of the room, blowing against the side of the head, the smell of a banana held to the nose, and other faint stimuli—often restore the alert state. A certain method of bringing the animal back to alertness is to place the finger tips over the nostrils for a few seconds so as to shut off the air supply. When the animal is alert, the recommencement of the experimental stimulation (let us say, movement of the visual field) will often reinstate the sleep-like condition. This connection between stimulation and the onset of the sleep-like state shows that it is not solely conditioned by the procedure of immobilization.

If, as the foregoing discussion indicates, the results of both Pavlov's experiments and our experiments are instances of competition between reaction systems, then the oft-named 'Laws of Conditioned Reflexes' achieved by his method are the laws of the competition of the reaction system of sleep with other active reaction systems such as food-taking. While these laws may have greater generality than that

particular situation, their extension to other situations, where different reaction systems are in conflict, must be made with caution.

V. Discussion

Competition between reaction systems.—To this point we have been guilty of over-simplifying the matters under consideration. We have proceeded as though the reciprocal relationship between response systems was always a relationship of *dominance* of one system over others. Actually, we must take into consideration a more complicated situation, for the complete dominance of one reaction system over another is but one of the modes of interaction of such systems. In general, there are five distinguishable ways in which two response systems may coöperate in the control of an effector which is under observation. These modes are not strictly separable, for one will probably be found to grade into the other by unclassifiable instances. Objective records of these five modes of interaction can be obtained from the movements of the eyes when the subject is reacting to simultaneous visual and vestibular stimulation. Other examples, from the competition of more complicated behavior systems, could easily be chosen.[9]

Allied systems:

(1) *Summation* or reinforcement. Illustrated by visually reinforcing a vestibular nystagmus by allowing the subject to see the surroundings while he is being rotated.

(2) *Synergic action.* Illustrated by superimposing a vertical visually induced nystagmus upon a horizontal vestibular nystagmus.

Antagonistic systems:

(3) *Algebraic summation.* Illustrated by the greatly reduced nystagmus resulting when a visual still-fixation stimulus is opposed against vestibular stimulation.

[9] Author's correction in proof: Since writing this paper I have become convinced that the selection of the terms "reaction system" and "response system" was unfortunate, inasmuch as these terms imply the need of a *stimulus* to evoke the system in question. Since many such systems owe their maintenance or origin to central factors, the term "behavior system" would be more accurate and would have greater generality.

(4) *Rivalry.* Illustrated by opposing horizontal vestibular nystagmus to the right against horizontal visually induced nystagmus to the left at certain relationships of speed of bodily rotation and rotation of the visual field so that neither system maintains dominance, but fluctuation takes place.

(5) *Dominance.* Illustrated by records of response to opposed visual and vestibular stimulation (as in 4) at such relationships of speed that one or the other achieves complete control of the final common path to the eyes.

With regard to the relationship of dominance we must consider two problems, or perhaps more accurately, two aspects of the same problem: (1) What factors keep one system dominant over others? (2) What factors cause a system to lose dominance? These questions state, in a very general form, the problem which faced us in connection with the behavior of our monkeys in a food-taking situation and are the questions which have given direction to our eye-movement experiments. For each particular situation the questions must have a particular answer, but certain generalizations can be made which aid in showing the direction of investigation.

When a reaction system is dominant, the factors which favor its continuance are the following:

(1) Natural prepotency.
(2) Stimulus strength.
(3) Reinforcement by other stimulus-response systems, whether these be from stimuli external to the subject or whether they be such factors as are ordinarily classed as attention, set or motivation.
(4) Absence of stimuli for competing reaction systems.

When two opposed response systems are in conflict, one of them being dominant and the other non-dominant, there are four ways in which the dominant behavior may be terminated (inhibited) with shift to the other system. These ways are:

(1) By removal of the stimulus of the dominant system, so that the non-dominant becomes manifest. This is a common enough situation illustrated by loss of response to

food after satiation and perhaps by the conflict of the approach and fear response systems described in section III above.

(2) Reinforcement of the non-dominant system so that it receives relatively the greater amount of excitement and becomes the dominant system.

(3) Fatigue or refractory period in the dominant system so that it becomes relatively less excitable than the non-dominant.

(4) Inhibition of the dominant system as an aspect of the excitation of some other system which is opposed to it, but is neutral or opposed to the non-dominant system. This situation may be illustrated by a hungry dog offered food by a stranger. Fear of the stranger remains dominant over the food-taking response until a pat on the head calls forth a different response to the stranger. The fear response is reciprocally inhibited by the pat on the head, and the food-taking response then gains dominance over both.

This analysis of the situation does not answer our earlier particular questions as to what the mechanism of the various types of conditioned inhibition may be. For instance, it does not tell us whether the loss of a conditioned response during experimental extinction and its replacement by other reaction systems is because of a depression of the former or an enhancement of the latter. That particular question must be answered, if possible, by experimental investigation, guided, however, by a more comprehensive view of the situation than is afforded from the Pavlovian stand.

Is all inhibition reciprocal?—The extension of the Principle of Reciprocal Innervation to the inhibitions of complex behavior means that we must take the point of view that anything an animal may be doing at the moment, be it sleeping, playing, grooming, vocalizing, sitting quietly, or any other, is reciprocally related to anything it would otherwise have been doing at the same moment. The responses which are absent or inhibited are so when the animal is responding to other stimuli, or when some other response than the one in question is being made to its stimulus. The identification of such reciprocal relationships is purely a matter of observed

dominance of one behavior system over another. Many kinds of responses can proceed contemporaneously, while others never do.

We have at present no means of proving conclusively, by experiment, the thesis adopted in this paper. It is quite probable that no crucial experiment, convincing to the opponents of this view, will be devised in the near future. Its acceptance rests on the convincingness of the argument that the higher functions should, wherever possible, be explained by principles established for the lower functions, on the gradual accumulation of instances (such as those in this paper) which are in harmony with the view, on its success in giving an orderly description of known facts, on its success in accounting for apparently negative instances (such as sleep), and on its usefulness for the anticipation of the results of experiments.

The neurophysiology of inhibition.—The Principle of Reciprocal Innervation applies to behavior. It does not require the acceptance or rejection of any of the present leading theories of the neural mechanism of inhibition. The proponents of refractory phase, *c.i.s.*, drainage and other theories have constructed more or less plausible schemes. The data here presented do not bear on these problems. Their solution will probably come from evidence of a quite different character. The confusion of the psychological problem with the neurophysiological problem has not added clarity to understanding or accuracy to description of observed behavior. The all-too-frequent association of the word 'inhibition' with special theories of its neural nature constitutes a real danger in its use. Some of these theories were devised to explain phenomena which have been incompletely or even inaccurately observed. Inhibition as an *independent* process may well be one such theory.

Positive and negative learning.—Negative learning (habituation, negative adaptation, extinction of conditioned reflexes, loss of behavior items in learning, etc.) is often treated as a separate problem from positive learning (conditioning, selection of correct response, etc.). An important

implication of the view presented in this paper is that positive and negative learning are not separable aspects of the problem of learning, but are always to be considered together. The loss of one mode of response is accompanied by a shift to another mode of response. Which aspect of the process we decide to record may in some cases be a matter of personal preference or convenience, but we must guard against basing our interpretations solely on the negative aspect. There is little enlightenment in recording what a subject does not do, so long as we wholly ignore what he does do.

VI. Summary

Starting with the assumption that behavior is organized into response systems, this paper presents new evidence for the point of view that the inhibition of complex behavior (for instance, the inhibition of conditioned reflexes) is one aspect of the process of competition between reaction systems. That is to say, an activity is inhibited, when some other reaction system takes its place. This view is contrasted with the Pavlovian view of inhibition as an independent process. It is shown:

(1) That the inhibition of vestibular nystagmus during habituation to rotation consists in the displacement of the vestibular eye-movement response system by another eye-movement response system. The character of this intruding system is briefly described.

(2) That in a conditioned food-taking-reaction experiment with monkeys the 'inhibition of delay,' 'extinctive inhibition,' 'internal inhibition,' and 'external inhibition' occur when other response systems natural to the experimental situation gain dominance over the food-taking response system. These other systems include manipulative and locomotor acts, play, grooming, fear, rage, etc.

(3) That Pavlov's laws of inhibition can be included under the more general laws of the competition of reaction systems.

A systematic description is given of the modes of interaction of reaction systems and of the factors which favor and which operate against the continuance of dominance of a reaction system.

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